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*Fig. 1.*

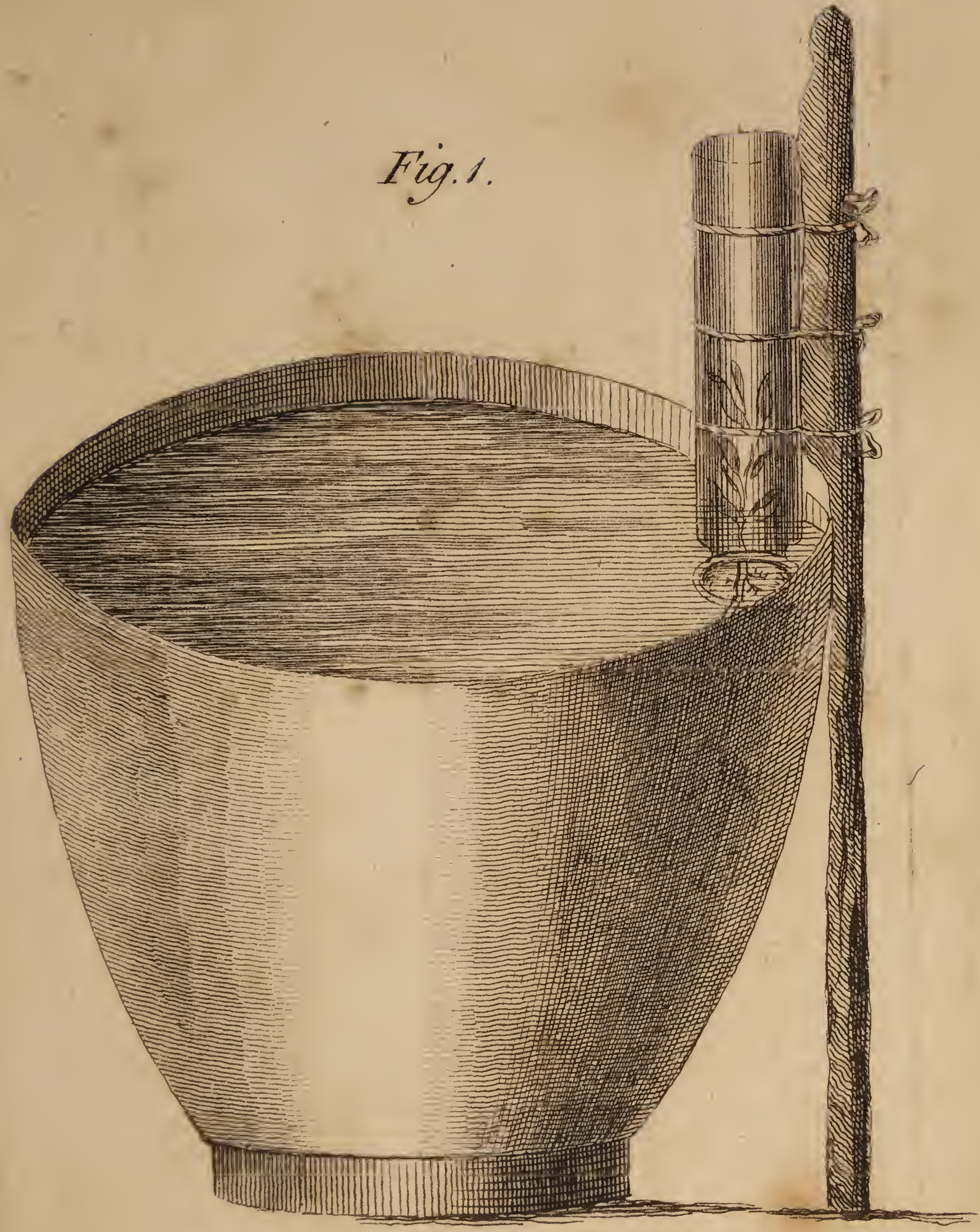
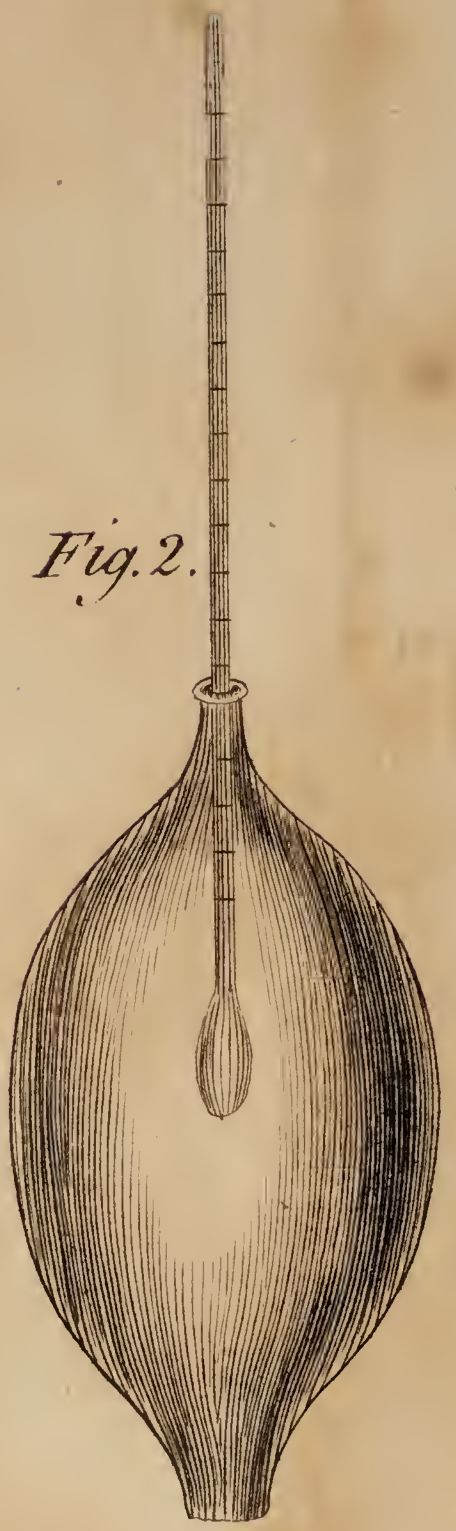


*Fig. 2.*



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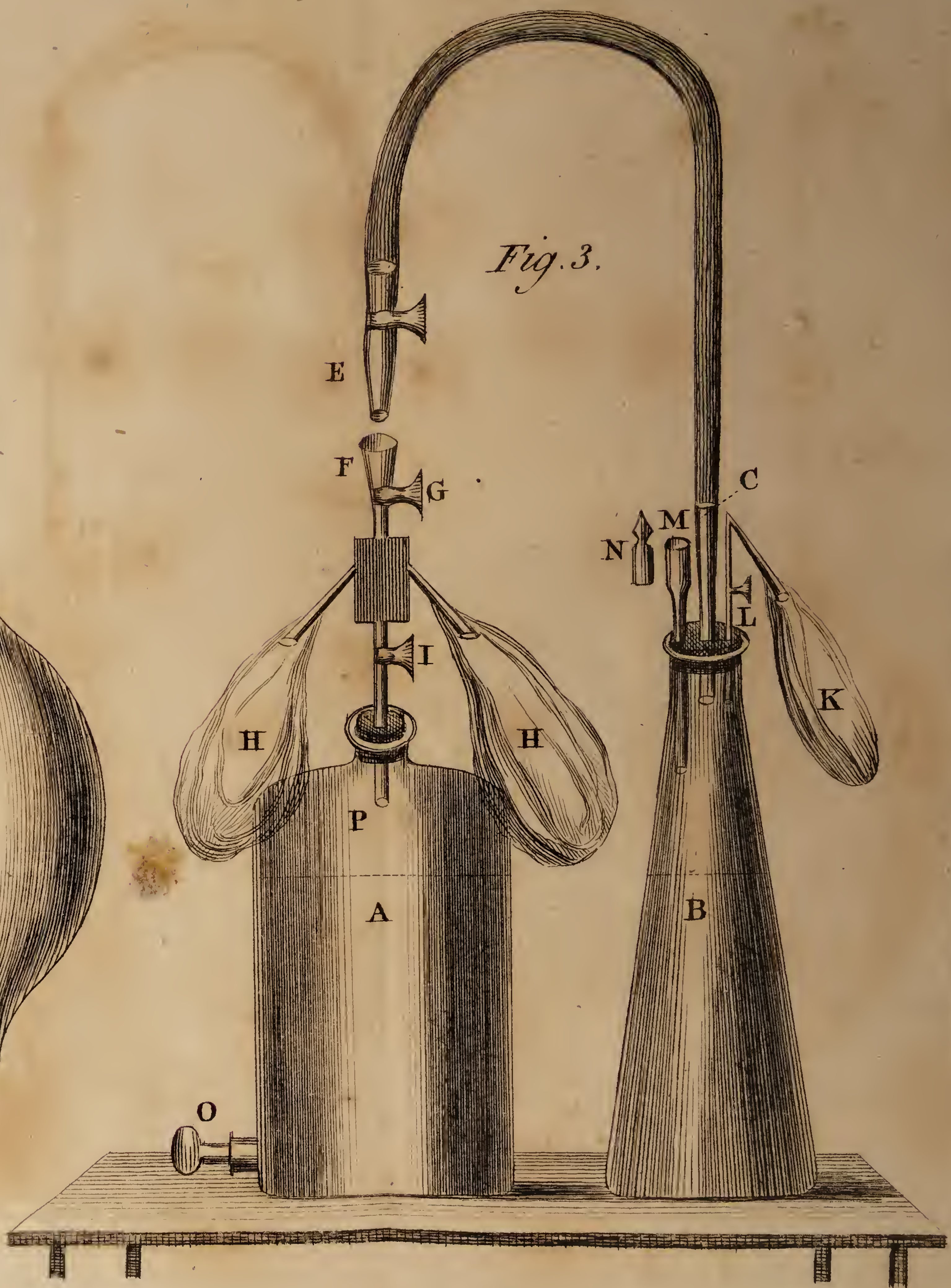

# EXPERIMENTS

AND

# OBSERVATIONS

RELATING TO VARIOUS BRANCHES OF

# NATURAL PHILOSOPHY;

WITH

# A CONTINUATION

OF

# THE OBSERVATIONS ON AIR.

The SECOND VOLUME.

---

By JOSEPH PRIESTLEY, LL. D. F. R. S.
Honorary Member of the Academy of Sciences at *Peterſburg*,
and of the Royal Academy of Medicine at *Paris*.

---

> Vires acquirit eundo.
>
> VIRGIL.

---

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# THE

# PREFACE.

THE progreſs I have made in my philoſophical purſuits ſince my laſt publication will not, I flattter myſelf, diſappoint the expectations of my reader, provided they be reaſonable. This volume, like all the preceding ones, contains a mixture of things of greater and of leſs importance, both with reſpect to the doctrine of *air*, and to ſubjects which have uſually been referred to the general head of chemiſtry.

He will find that I have proſecuted with conſiderable ſucceſs the experiments on the *green vegetable matter*, of which I treated in my laſt volume; nothing having been then wanting, but the certainty of this being *a vegetable ſubſtance*, to my advancing one conſiderable ſtep in tracing the manner in which this and other growing vegetables purify noxious air, viz. by means of the action of *light* upon them; having then ſufficiently ſatisfied myſelf, that it was *light* only, and not *heat*, that enabled this ſubſtance, whatever it was, to yield pure air. And from this I have been led to determine other important circumſtances relating to the general œconomy of nature. The reader will alſo find other new facts relating to the ſeveral kinds of air, which give new views of their conſtitution.

On this account I had intended to have drawn out a *general theory* of all the kinds of air, and *tables of affinity*, to explain the various phænomena in which they are concerned. But having many things yet to aſcertain before I can ſatisfy myſelf with reſpect to this ſubject, and being unwilling to obtrude any crude conceptions on my readers, I have thought proper to defer it till I ſhall have made ſome farther progreſs in my inquiries.

However, the ſummary and methodical view that I have given of all the *facts* that I have diſcovered will aſſiſt any perſon to form as good a theory as I could for him. For all that is properly meant by *a theory*, excluſive of *hypotheſis*, is a number of general propoſitions, comprehending all the particular ones, deduced from ſingle experiments. A general theory will alſo be the leſs neceſſary on account of my having made all the uſe I could of the moſt important new facts, in obſerving, at the time that I mention the diſcoveries, how much they ſeem to affect our ideas of the conſtitution of the particular kinds of air to which they relate.

This volume would probably have been larger, or have made its appearance earlier, but that the former part of the laſt ſummer was engroſſed by a tedious and dangerous illneſs, and the latter part of it was taken up by my removal from Calne to Birmingham. But my health being, by the goodneſs of God, in a good meaſure re-eſtabliſhed, I flatter myſelf I ſhall be able to devote myſelf as much as in any former period of my life to philoſophical purſuits; and during my incapacity for making experiments, my hints for the farther proſecution of them are greatly accumulated. But whether my application to theſe ſtudies will be crowned with *ſucceſs* is a thing for which I cannot anſwer. I can only promiſe to follow with aſſiduity ſuch lights as have occurred, or that may occur to me.

It has been one conſequence of the late interruption of my philoſophical purſuits, that many courſes of experiments recited in this volume are left imperfect. But whenever the publication had been made, this muſt, from the nature of the thing, have been, in a great meaſure, unavoidable. No philoſophical inveſtigation can be ſaid to be completed, that leaves any thing unknown that we are prompted by it to wiſh we could know relating to it. But ſuch is the neceſſary connection of all things in the ſyſtem of nature, that every diſcovery brings to our view many things of which we had no intimation before, the complete diſcovery of which we cannot [redacted] help wiſhing for; and whenever theſe diſoveries are completed, we may aſſure ourſelves they will farther increaſe this kind of diſſatisfaction.

The greater is the circle of light, the greater is the boundary of the darkneſs by which it is confined. But, notwithſtanding this, the more light we get, the more thankful we ought to be, for by this means we have the greater range for ſatisfactory contemplation. In time the bounds of light will be ſtill farther extended; and from the infinity of the divine nature, and the divine works, we may promiſe ourſelves an endleſs progreſs in our inveſtigation of them: a proſpect truly ſublime and glorious. The works of the greateſt and moſt ſucceſsful philoſophers are, on this account, open to our complaints of their being imperfect. Did Dr. Hales excite no wiſh that he did not ſatisfy with reſpect to *air?* And did Newton himſelf ſolve every query concerning *light?*

I am ready, however, to acknowledge, that more experiments (which the ſagacious reader will ſee I might have made) would probably have carried me conſiderably farther in ſeveral of the ſubjects of inquiry. But having materials enow for a volume, and entering, as I may ſay, upon *a new period of life*, I was willing to cloſe my philoſophical accompts as they ſtand at preſent, before I open a new one. And whenever I have made any publication, I have been unwilling to with-hold from my reader any thing concerning which I was able to give him even imperfect information. By this means he is fully poſſeſſed of all that I myſelf know on the ſubject, and is therefore equally qualified to ſatisfy his own impatience, in whatever reſpects I may have left it unſatisfied. He cannot but ſee, if he conſiders what is now before him, that many new and promiſing fields of inquiry are open to us. And I thank God I am now in a ſituation in which I have every advantage for entering upon them.

Having ſufficient room for it, I have introduced into this volume the paper on the *lateral exploſion*, from the Philoſophical Tranſactions of the year 1760. By this means every thing that I have written on philoſophical ſubjects will be found in theſe five volumes, or in my *Hiſtory of Electricity*. There only remains in the Philoſophical Tranſactions, a few papers of mine containing general reſults of proceſſes, the details of which will be found in this work.

I have alſo addreſſed *a Letter to Mr. Kirwan* on his Notes to *Mr. Scheele's treatiſe on air and fire*. But it contains little more than will be found at large in this volume, except an opinion which I have long entertained, that the exploſive power of *aurum fulminans* is owing to the ſetting at liberty a great quantity of alkaline air, an opinion which I have not yet verified by experiment, but of which I entertain little doubt.

The method I have uſed in this volume is the ſame with that of the preceding, viz.

*analytic* and *hiſtorical*, but as conciſe as poſſible. Details of many experiments will always be tedious to thoſe who look for nothing but general reſults; but to thoſe who chuſe to proſecute the inquiries themſelves, they will be found abſolutely neceſſary. I ſhall myſelf be obliged to look back to all the circumſtances that I have noted, whenever I reſume the inveſtigations; and I therefore conclude they will be of the ſame uſe to others. My wiſh is not only to gratify the generality of thoſe perſons who take pleaſure in reading philoſophical writings, but alſo to aſſiſt future experimenters. Others may content themſelves with ſeeing the concluſions, and paſs by the details.

It has neceſſarily happened, from my having publiſhed my experiments as ſoon as the hiſtorical account of them would ſupply materials for a volume (a conduct with which I ſee every reaſon to be ſatisfied) not only that ſome things required to be proſecuted farther, but that others have required *correction*. On this account, all thoſe who now read the whole work from the beginning, will wiſh to be informed, as they proceed, how far the articles under their immediate conſideration have received illuſtration from the farther proſecution of the ſame ſubjects, of which an account is given in the ſubſequent volumes.

For this reaſon I conceived that it muſt be uſeful, in this advanced ſtate of the work, to reviſe the whole from the beginning, and write notes on thoſe paſſages on which I can throw more light than I was able to do at the time when they were written. Such notes I have now drawn up. They relate chiefly to the *firſt volume.* In future time, myſelf, or others, may be able to throw as much light on the imperfect obſervations in the later volumes.

It has alſo happened from the publication of this work at different periods, that the arrangement of the whole, as *one work,* could not be regular; ſome things having been diſcovered laſt, which, in an orderly ſyſtem, ought to have been placed firſt; and many things nearly allied in nature being widely diſtant from each other; a defect which no *Index*, in the uſual form, can ſufficiently remedy.

For this reaſon I have alſo thought it would be uſeful (and indeed I have been frequently urged to it by my friends) to draw up a *methodical index*, or *a ſummary view of all the more important facts*, with references to thoſe parts of the work in which they are more largely treated of. This I have now done with ſome care, but in as few words as poſſible, the moſt general reſults only being recited, referring to the work itſelf for all details and proceſſes. A few things, however, are repeated, when they equally came under two different heads, and were of ſufficient importance. Such a ſummary view as this will be particularly uſeful to ſuggeſt what is yet moſt wanted to extend this branch of knowledge, and to direct the views of future experimenters.

I cannot conclude this Preface without expreſſing my joy that the progreſs of philoſophy does not ſeem to be much retarded by the calamities of war, in which a great part of Europe is unhappily involved; except that the correſpondence of philoſophers in diſtant parts of the world is interrupted by it, and a greater difficulty is found in procuring foreign publications.

The principal ſubject of this volume is purſued with great ardour by many foreigners, as well as by philoſophers in this country; and ſince the character that Virgil gives to *Fame* is ſtill more applicable to *Philoſophy*, *vires acquirit eundo* (which I have taken for a motto to this volume) we may flatter ourſelves with a proſpect of a rapid progreſs in diſcoveries of the moſt intereſting nature.

I alſo cannot help expreſſing a wiſh that with the return of peace (and happily it is not in the power of any ſtate to be always at war) we may ſee every obſtruction to the progreſs of knowledge, which is equally friendly to all ſtates, removed. Taxes on the importation of books, and other articles of literature, are ſo impolitic, as well as illiberal, that it is earneſtly wiſhed that ſomething may be ſtipulated by the contending powers for aboliſhing them. There are ſtateſmen whoſe minds are ſufficiently enlarged to ſee that Philoſophy gives an ample equivalent for the exemption.

*Birmingham, 24th March*, 1781.

THE

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## THE

# INTRODUCTION.

HAVING appropriated the *Introduction* of each of the former volumes to the deſcription of apparatus, and an account of improvements in the method of making experiments, and having done little of this kind ſince my laſt publication, an Introduction to this volume was hardly neceſſary. It may be worth while, however, to give a ſhort account of the *earthen jar* in which I made many of the experiments on the growth of plants in different kinds of air, recited in this volume; and a bare inſpection of *fig.* 1, will be almoſt ſufficient for this purpoſe.

The jar was about eighteen inches in diameter at the top, and of the ſame depth.

It was placed in an open expoſure in the garden, and ſticks were thruſt into the earth in a perpendicular poſition, quite round it; and to theſe ſticks glaſs jars, filled with water, with their mouths inverted in the water of the earthen jar, were faſtened by ſtrings. After I had introduced into one of theſe jars any particular kind of air, I afterwards drew through the water, and put into it, any plant, the top and leaves of which I wiſhed to expoſe to it; ſupporting the root or ſtalk at a proper height in the earthen jar, if I found that any ſuch ſupport was neceſſary. In ſome caſes in will be found that the top of the plant was in one jar, and the root or ſtalk in another; which it was not at all difficult to do.

*Fig.* 2, repreſents the inſtrument by which I endeavoured to aſcertain the conducting power of different kinds of air with reſpect to heat. It conſiſts of a glaſs bulb open at both ends, ſo that I could eaſily faſten a thermometer with its bulb in the center of it, where it would be ſurrounded by any kind of air, introduced into it after it had been previouſly filled with mercury. The manner in which the experiments were made is ſufficiently deſcribed in the account of them, p. 375.

I have made ſeveral improvements in my method of making experiments, but they are not of importance enough to deſerve a particular deſcription. If I ſhould make any farther alterations, I may perhaps, on ſome future occaſion, give a drawing and deſcription of my whole apparatus, according to the lateſt improvements that I ſhall be able to make in it.

It may be proper in this place to remind my reader that, in meaſuring the purity of reſpirable air, I mix with it an equal quantity of nitrous air, or if it be highly dephlogiſticated, two equal quantities of nitrous air, which is always particularly mentioned in the courſe of the volume: after this I transfer the mixture into a graduated tube. Conſequently a leſs number in the reſult is always an indication of greater purity. This number, in order to be as conciſe as poſſible, I have in this volume termed *the meaſure of the teſt*. Thus if when I mix two equal quantities of common air and nitrous air, they afterwards occupy the ſpace of one meaſure, and two tenths of a meaſure, I ſay *the meaſures of the teſt were* 1. 2.

# OBSERVATIONS
# RELATING TO
# VARIOUS BRANCHES
# OF
# NATURAL PHILOSOPHY.

## SECTION I.

## *Of the growth of the* WILLOW PLANT *in different kinds of air.*

IN my laſt publication I obſerved that the willow plant grew very well both in inflammable and in common air, and that it abſorbed a conſiderable proportion of both the kinds, as well as of nitrous air. In this there could not poſſibly be any miſtake, unleſs we ſuppoſe the water to have abſorbed the air, which it has never been known to do in any ſimilar circumſtances. However, when I reſumed the experiments on the growth of this plant in the courſe of the laſt ſummer, I had no inſtance of the abſorption of common air; but I had repeated, and very extraordinary ones of the abſorption of inflammable air by it, and the plant flouriſhed ſo remarkably in this air, that it may be ſaid to feed upon it with great avidity. This proceſs terminates in the change of what remains of the inflammable air into phlogiſticated air, and ſometimes into a ſpecies of air as good as common air, or even better; ſo that it muſt be the *inflammable principle* in the air that the plant takes, converting it, no doubt, into its proper nouriſhment.

Some other plants alſo, as *Comfrey* and *duck-weed*, I obſerved to thrive very well in inflammable air, and to produce a ſimilar effect upon it, though, as I obſerved in my firſt publication on the ſubject of air, and upon other occaſions ſince, *mint* does not thrive ſo well in this as in common air, and I have generally found that, in time, this plant is killed by it.

It may deſerve to be mentioned in this connexion, that the willow plant grows beſt in marſhy places, which abound with inflammable air. The plants that I made uſe of grew in the bottom of a field, in and near a piece of water, into which, if I only thruſt a ſtick, a prodigious quantity of inflammable air ruſhed out, ſo that, without changing my place, I could, at any time, collect a large receiver full of it; and bubbles of air were very frequently riſing ſpontaneouſly from the mud at the bottom. It may, therefore, be a proviſion in nature, that this noxious kind of air ſhould be fitted to the nouriſhment of ſuch plants as grow beſt in thoſe places in which it abounds, as well as that plants in general ſhould purify the common atmoſphere.

The facts from which theſe concluſions are drawn, as well as ſome farther obſervations on the ſubject, are the following, in the recital of which it will be neceſſary to mention the month and the day on which the obſervations were made, as they have a connexion with the ſtate of the plant, and probably with its powers of action on air.

On the 26th of May 1779, I put a jar of about twenty ounce meaſures of air over a willow plant growing in water, and on the firſt of June I obſerved that the air was little diminiſhed in quantity, or affected in quality; for by the teſt of nitrous air the meaſures were 1.33; that is, when one meaſure of nitrous air was mixed with one meaſure of this air, they occupied the ſpace of 1.33 meaſures. The plant continuing to grow, I examined it on the 5th of June, when the meaſures were 1.3, and thoſe of the common air at the ſame time, I obſerved, were 1.26. This ſlight degree of injury I imputed to ſome black leaves, which were then about the plant. On the 8th of the ſame month, the meaſures were 1.36, and on the 15th they were 1.4; and there was ſtill no more proſpect of the air being abſorbed than before. This I thought very extraordinary, as in the preceding ſummer I had always found, take what care I could, that theſe plants injured common air, and at leaſt diminiſhed it in the uſual degree of one fourth, if they did not abſorb more of it.

In inflammable air the reſults were conſiſtent with the preceding obſervations, and uniform with themſelves. But the year before I had no opportunity of purſuing theſe obſervations to the extent that I wiſhed, ſo that I could not tell in what ſtate the plants would finally leave the air; whereas now I had ſufficient time fully to ſatisfy my curioſity in this reſpect.

On the 18th of May I introduced one of theſe plants, growing in water, under a jar of ſtrong inflammable air, and the 3d of June following, I found that it was diminiſhed about one third. Examining it, I found it was but weakly inflammable. This plant had not room to expand itſelf, but ſtill it lived very well. On the 31ſt of the ſame month, there was no more than one third of the air remaining in the jar, and it was ſtill ſlightly inflammable. Owing to ſome accident or other, the plant had been dead about a week, after which time I obſerved that the air had ceaſed to be diminiſhed.

I then introduced another plant into what remained of the air, and on the 5th of June it was reduced one third more, and then I could not perceive that there was any thing inflammable in it. It was alſo a good deal dephlogiſticated; for with two equal quantities of nitrous air, the meaſures of the teſt were 1.6, ſo that, upon the whole, the growth of this plant in this kind of air had the ſame effect upon it as agitation in water would have had, viz. diminiſhing it, depriving it of its inflammability, and rendering it in ſome meaſure reſpirable.

I had another reſult exactly correſponding with this. For on the 9th of June I examined a quantity of inflammable air, in which a willow plant had grown from the 26th of May; but in this caſe not more than about half the quantity was abſorbed, but part of the remainder fired with one exploſion, like a mixture of common and inflammable air; and applying the teſt of nitrous

trous air, the meaſures were 1.43, which is about that ſtate of air in which a candle juſt goes out. On the 15th of June another quantity of this air, in which a willow plant had grown from the ſame time, was fired in the ſame manner, and the meaſures of the teſt were 1.44; though only about one half of it had diſappeared.

Another quantity of the ſame kind of inflammable air, in which a willow plant had grown the ſame time, was reduced to one ſixth of its original quantity. It then exploded like a mixture of common and inflammable air, and the meaſures of the teſt were 1.53. It was examined on the 9th of June. On the 15th of the ſame month it was diminiſhed ſtill more, and had then nothing inflammable in it, but the purity was nearly the ſame; the meaſures of the teſt being 1.54.

In all the experiments that I made of this kind, the quantity of air abſorbed was very various, depending probably, upon the health of the plant, its ſize in proportion to that of the jar, and other circumſtances.

On the 24th of May I had introduced one of theſe plants into a jar of inflammable air, collected from the marſh near which I had gathered it; and on the 9th of June I found it ſo far diminiſhed, that little more than one ſeventh of the original quantity remained. This was merely phlogiſticated air; for it was not affected by nitrous air, and extinguiſhed a candle.

On the 15th of June, I found that another quantity of the ſame kind of inflammable air, in which a willow plant had grown from the ſame date, was not diminiſhed near ſo much; for about one third of the original quantity was left. This, however, was partly inflammable, the ſlighteſt blue flame imaginable being perceived in a large jar of it. When I applied the teſt of nitrous air, the meaſures were 1.62.

A ſign of the great vigour of the plants growing in inflammable air, was the vivid greenneſs not only of the leaves that were in the air, but of thoſe alſo that were under water, and the length of time that they continued ſo in theſe circumſtances; whereas, in general, when the top of the plant was in common air, the leaves that were under water ſoon became diſcoloured, and periſhed. Theſe leaves on the contrary, not only continued green, but were always loaded with air bubbles, which were continually detaching themſelves, and riſing into the jar, having their places ſupplied by others. Theſe bubbles, I had no doubt, conſiſted of the air that had been ſtrained, as it were, through the plant, leaving its phlogiſton behind, for the nouriſhment of the plant. I endeavoured to collect a quantity of theſe bubbles, before they mixed with and diluted the inflammable air in the top of the jar, but I did not ſucceed. I have no doubt but that it would have been dephlogiſticated air, as this will eaſily account for the ſtate in which I found this air in the experiments recited above.

It was doubtful, however, whether theſe bubbles conſiſted of air that had been imbibed by the leaves, and then paſſed through a conſiderable ſpace within the ſubſtance of the plant, or of the air that had been contained in the water, to which theſe leaves had immediate acceſs. The latter ſeems more probable from ſome experiments, but the following are nearly deciſive in favour of the other ſuppoſition.

I put the ſtalk of a willow plant into an inverted jar full of water, while the top of it was in a jar of inflammable air. In theſe circumſtances a ſmall quantity of air was collected in the inverted jar, and it was evidently better than common air. This air I had obſerved to come from all the outſide of the ſtalk, but eſpecially from the places where the leaves had been broken off; and there were ſome few bubbles from the middle of the place where the ſtalk itſelf had been cut, for it had no root.

In another experiment of this kind, when the plant had been in the ſituation above deſcribed, from the 11th to the 14th of June, three fourths of an ounce meaſure of air was collected in the inverted jar, ſo pure, that the meaſures of the teſt were 0.63; and with two meaſures of nitrous air 1.5. Applying the flame of a candle at the orifice of a tube filled with it, there was a loud exploſion, ſo that it was a mixture of dephlogiſticated and inflammable air.

On the 19th of June, I collected half an ounce meaſure more from the ſame plant; and applying to it the teſt of nitrous air, the meaſures were 0. 9, and there was nothing ſenſibly inflammable in it. Had there been nothing inflammable in the air collected in the inverted jar, containing the ſtalk of the plant, the probability would have been, that all the air came from the water, dephlogiſticated by the action of the plant; but the mixture of inflammable air in it ſeems to prove that part of it, at leaſt, had been imbibed by, and ſtrained through the plant, entering at the leaves (which alone were expoſed to the inflammable air) and iſſuing at the ſtalk, which was turned up into the other jar in which the air was received. This ſingular caſe, for it is the only reſult I ever had of the kind, ſhows that the plant had taken in more nouriſhment than it could properly digeſt.

This plant thriving ſo remarkably well in inflammable air, and depriving it of its inflammability,

flammability, I thought it could not well fail to purify phlogiſticated air, if I gave proper attention to its health and eaſe in its confined ſituation, though (perhaps through want of this attention) it had failed to do ſo the preceding ſummer; and I was not diſappointed in my expectations at this time.

On the 22d of June I introduced one of theſe plants into a jar of air phlogiſticated by the putrefaction of fiſhes, confined by rain water, in which I had found by frequent trials, that the green vegetable matter was not ſoon generated; and on the 26th of the ſame month, it was ſo much improved, that the meaſures of the teſt were 1. 38, which is a little better than the ſtate in which air will juſt extinguiſh a candle. The 3d of July, I examined it again, and then the meaſures were 1. 32, and on the 15th of the ſame month, it was exactly of the ſtandard of common air. The water by which it was confined certainly produced no air; for another jar filled with water, in the ſame trough, and therefore having preciſely the ſame expoſure with reſpect to light, and all other circumſtances, had no air at all in it. A very little air was ſtrained through this plant, and it was almoſt thoroughly phlogiſticated; for the meaſures of the teſt were 1. 7.

Nitrous air I have always found to be fatal to vegetable, as well as to animal life, and ſo it proved in this inſtance; as indeed it had done the preceding ſummer. From the 18th of May to the 18th of June, a quantity of this air was diminiſhed by a willow plant to one fourth, and then it was ſo changed, that it admitted a candle to burn in it with a gently blue enlarged flame; a ſtate which, as I have obſerved, nitrous air generally paſſes through before it becomes mere phlogiſticated air, and which, in the courſe of this volume, will appear to be nitrous air partially dephlogiſticated.

Phlogiſton being the pabulum of plants, as it is probably of animals too, dephlogiſticated air muſt (as indeed I had found before) be unfavourable to the growth of plants in general; and I conſtantly found it to be ſo in the caſe of the willow plant. To give it the fairer trial, I introduced a ſmall but healthy plant, growing in the marſh, into a jar of this air, ſo large that the plant was not in the leaſt incommoded, and it did not reach the top of the jar by ſeveral inches. This was done the 18th of May, but it died preſently, and before the air was ſenſibly diminiſhed; which was the caſe afterwards, owing, probably, to the putrefaction of the plant. But even then, being examined with two equal quantities of nitrous air, the meaſures of the teſt were 1. 0.

Having filled a large earthen pot with water, and having ſticks thruſt into the earth quite round it, for the convenience of faſtening jars with their mouths inverted in water, in order to fill them with different kinds of air, and introduce plants into them, without the trouble of going to the marſh in which they grew; I filled one of theſe jars with dephlogiſticated air, and then introduced the top of a willow plant into it. In a day or two, all the part that was within the jar began to turn white, and was ſoon after manifeſtly quite dead, even when many ſhoots of the ſame plant that were under water continued green, and looked well a conſiderable time afterwards. The air, being examined, was found to be very little injured. I therefore think we may ſafely conclude, that dephlogiſticated air is univerſally hurtful to plants; and this, *a priori*, would be an argument in favour of the depuration of atmoſpherical air by vegetation.

Having made the preceding experiments on inflammable air with the willow plant, I proceeded to try a few other plants; and without giving ſuch particular attention to theſe as to thoſe of the willow plant, I ſoon found that *comfrey*, which is hairy like the willow plant, and grows beſt in the ſame ſituation, and alſo the *meadow ſweet* grew very well in inflammable air. So alſo did *duckweed*, which was always remarkably healthy, and of a deep green colour, a certain ſign, I believe, of health and vigour in plants in general; whereas, in dephlogiſticated air, duckweed always preſently became pale, and died.

## SECTION II.

### *Of the purification of air by plants and the influence of light on that proceſs.*

ONE of my earlieſt obſervations on the ſubject of air, but made caſually, when, in fact, I expected a contrary reſult from the proceſs, was the purification of air injured by reſpiration or putrefaction, by the vegetation of plants. But at that time I was altogether ignorant of the part that *light* had to act in the buſineſs. At the publication of my laſt volume, I had fully aſcertained the influence of light in the production of dephlogiſticated air in water by means of a *green ſubſtance*, which I at firſt ſuppoſed to be a plant, but not being able to diſcover the form of one, I contented myſelf with calling it ſimply *green matter.*

Several of my friends, however, better ſkilled in botany than myſelf, never entertained any doubt of its being a plant; and I had afterwards the fulleſt conviction that it muſt be one. Mr. Bewly has lately obſerved the regular form of it by a microſcope. My own eyes having always been weak, I have, as much as poſſible, avoided the uſe of a microſcope.

The principal reaſon that made me queſtion whether this green matter was a plant, beſides my not being able to diſcover the form of it, was its being produced, as I then thought, in a phial cloſe ſtopped. But this being only with a common cork, the ſeeds of this plant, which muſt float inviſibly in the air, might have inſinuated themſelves through ſome unperceived fracture in it; or the ſeeds might have been contained in the water previous to its being put into the phial. Both Mr. Bewly and myſelf found, in the courſe of the laſt ſummer, that when diſtilled water was expoſed to the ſun, in phials filled in part with quickſilver, and in part with diſtilled water, and inverted in baſons of quickſilver, none of this green matter was ever produced; no ſeed of this plant having been able to penetrate through the mercury, to reach the water incumbent upon it, though, in ſeveral caſes, it will be

ſeen,

ſeen, that theſe ſeeds diffuſe and inſinuate themſelves, in a manner that is truly wonderful.

Without light, it is well known, that no plant can thrive; and if it do grow at all in the dark, it is always white, and is, in all other reſpects, in a weak and ſickly ſtate. Healthy plants are probably in a ſtate ſimilar to that of *ſleep* in the abſence of light, and do not reſume their proper functions, but by the influence of light, and eſpecially the action of the rays of the ſun. This was the reaſon why no green matter was ever produced by means of mere *warmth* in my former experiments, and that in jars ſtanding in the ſame expoſure, but covered ſo that the light had no acceſs to them, no pure air was collected, none of the green matter being then found in them.

This I verified moſt completely by covering the greateſt part of a glaſs jar with black ſealing-wax, which made it thoroughly opaque; and beſides anſwering that purpoſe better than brown paper, as I made the experiment mentioned in my laſt volume, did not imbibe any of the water, and therefore did not promote the evaporation of it. To be able to obſerve whether any air was collected in theſe jars, or not, the upper part of them was not coated with ſealing-wax, but had a thick moveable cap of paper, which I could eaſily take off, and then inſpect the ſurface of the water.

In order to ſatisfy myſelf as fully as poſſible with reſpect to this remarkable circumſtance, I alſo made the following experiments, the reſult of which are, indeed, very deciſive in favour of the influence of *light* in this caſe.

Having a large trough of water, full of recent green matter, giving air very copiouſly, ſo that all the ſurface of it was covered with froth, and jars filled with it, and inverted, collected great quantities of it, and very faſt; I filled a jar with it, and, inverting it in a baſon of the ſame, I placed it in a dark room. From that inſtant no more air was yielded by it, and in a few days it had a very offenſive ſmell, the green vegetable matter with which it abounded being then all dead, and putrid.

Again, having filled a receiver with freſh pump water, and having waited till it was in a ſtate of giving air copiouſly, I removed it into a dark room; and from that time the production of air from it intirely ceaſed. When I placed it again in the ſun, it gave no air till about ten days after, when it had more green matter, the former plants being probably all dead; and no air could be produced till new ones were formed.

With the ſame view I placed ſome ſmall ſlices of *roaſted beef* in a veſſel of water in the ſun; and an equal quantity, in another veſſel of the ſame ſize, in the dark; when the former became green, and yielded air, but the latter not at all. It will be ſeen afterwards that many animal ſubſtances afford an excellent pabulum for this green vegetable matter.

I alſo made a ſimilar experiment with ſlices of *cucumber*, when thoſe in the ſun became covered with green matter, and yielded pure air, but thoſe that had been placed in the ſhade, though they did yield a ſmall quantity of air, it was wholly phlogiſticated, though not inflammable, which many vegetable ſubſtances yield in the ſame circumſtances.

That it was the *green matter*, that yielded the air, and not the mere action of *light* upon the water, might be inferred from my former experiments; and this was my own idea at the firſt, though I quitted it afterwards. The appearance which then miſled me was the great quantity of pure air emitted by the water, after it was poured off from the green matter. But before any air can appear on the ſurface of the water, in its elaſtic ſtate, the water itſelf muſt be thoroughly ſaturated with it, in which caſe it contains ſo much air, that, upon the leaſt agitation, even without heat, it readily parts with it, and exhibits the beautiful appearance which I then deſcribed. But that, notwithſtanding this appearance, it was the *green matter*, and not the *water* that yielded the air, I was convinced by the following experiment.

Having a number of earthen plates covered with green matter, I introduced ſeveral of them under veſſels filled with freſh pump water, and then placed them in the ſun, together with other veſſels filled with the ſame water, at the ſame time, but ſtanding on clean plates; when I conſtantly found that air was immediately produced in the veſſels containing the green matter but none in the others, till the green matter was naturally formed in them; after which, but not before, pure air was produced in thoſe veſſels alſo.

I likewiſe uſed water that had long been expoſed to the ſun's light, ſo that it muſt have depoſited every thing that mere *light* could make it part with; and yet in this water, upon plates of green matter, air was immediately produced, as well as in water that had never been expoſed to the ſun.

I was led to theſe experiments by obſerving that air was immediately produced from thoſe parts of my jars to which green matter from former experiments happened to adhere, not having been carefully cleaned. It was likewiſe an evidence that it was the green matter, and alſo in a vegetating ſtate, that yielded the air, that when a plate covered with it had been made pretty hot before the fire (by which the plants had probably been killed) it was incapable of yielding any air.

Having, by this means, fully ſatisfied myſelf, that the pure air I had procured was not from the *water*, but from the green vegetating ſubſtance aſſiſted by light, I concluded that other *aquatic plants* muſt have the ſame effect; and going to a piece of ſtagnant water, the bottom of which was covered with ſuch plants, I took five or ſix different kinds promiſcuouſly. Then having put them into ſeparate jars of the water in which they were growing, and inverted them in baſons of the ſame, I placed them in the ſun; and I found that all of them, without exception, were immediately covered with bubbles of air, which gradually detaching themſelves from the leaves and ſtalks, where they had originated, roſe to the ſurface of the water; and this air, being examined, appeared to be, in all the caſes, very pure, though not quite ſo pure as that which I had before procured from the green matter; the meaſures of the teſt, with two equal quantities of nitrous air, being, at a medium, 1.5. Afterwards air procured from theſe plants was very nearly as pure as the other.

In order to aſcertain with more preciſion the real origin of this pure air, and eſpecially to determine whether it was properly *produced* by the light, and ſomething within the plant (which, as I found afterwards, ſeems to be the idea of Dr. Ingenhouſz *) or only by dephlogiſticating the air previouſly contained in the water, which I ſuſpected from my former experiments on vegetation, I kept a quantity of theſe water plants in jars of water in the ſun, as long as they would give any air; then only changing the water I found that the ſame plants immediately began to give freſh air as copiouſly as at firſt. The particulars of the experiment were as follows.

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* He ſays (Experiments on vegetables p. 23) that "the "air obtained from the leaves is by no means air from "the water, but air continuing to be produced by a ſpe-"cial operation carried on in a living leaf, expoſed to "the day light, and forming bubbles, becauſe the ſur-"rounding water prevents this air from being diffuſed "through the atmoſphere." Again, p. 89, he ſays of the green vegetable matter, "It is wonderful that this "green matter ſeems never to be exhauſted of yielding "dephlogiſticated air, though it has no free communica-"tion with the common atmoſphere, from which the "moſt part of other plants ſeem to derive their ſtock of "air. Does this vegetable matter imbibe this air from "the water, and change it into dephlogiſticated air? "this does not ſeem to me probable----I ſhould rather in-"cline to believe that the wonderful power of nature, "of changing one ſubſtance into another, and of promot-

I put a handful of theſe water plants, without diſtinguiſhing their kinds, into a receiver containing eighty ounce meaſures of water, inverted in a baſon of the ſame; and when they had yielded between ſix and ſeven ounce meaſures of air, I examined it, and found that with two equal quantities of nitrous air, the meaſures of the teſt were 0.8. But the air had been diminiſhing about three days, ſo that I believe there had been eight ounce meaſures in all, or one tenth of

" ing perpetually that tranſmutation of ſubſtances which
" we may obſerve every where, is carried in this green
" vegetable matter in a more ample and conſpicu-
" ous way."

the capacity of the jar, and certainly purer than it was now found to be. It was evident, therefore, that no more air would have been produced by theſe plants in this water, though placed in the ſun. Replacing this jar with more of the ſame river water, the ſame plants were inſtantly covered with air bubbles, and in a very few hours had yielded more than an ounce meaſure of air. Some *duck weed*, which ſwam on the top of the water, in the former part of this experiment, was dead, owing, no doubt, to the purity of the air to which it had been expoſed,

To conclude this ſeries of experiments, I expelled air from a quantity of this river water, both before the plants were put into it, and afterwards; and I found that the air contained in it was purer after the plants had been confined in it than before, though the whole piece of water being quite full of plants, the air contained in it was tolerably pure in the firſt inſtance. The meaſures of the teſt, with an equal quantity of nitrous air being 1. But the air expelled from the water after the plants would grow in it no longer, was ſo pure, that with *two* equal quantities of nitrous air, the meaſures of the teſt were 1. Alſo, whereas the phial of water in the firſt caſe gave only 2.4 ounce meaſures of air, the ſame phial afterwards gave 4.4; which is nearly twice as much. For, as I have obſerved before, whereas the phlogiſtication of air diminiſhes the quantity of it, the dephlogiſtication muſt increaſe the quantity; and this increaſe exceeding the quantity which the water is capable of holding in ſolution, part of it is detached, and appears in an elaſtic form on the ſurface.

It is alſo a proof that the proper origin of all the air produced in theſe circumſtances is not the plant and the light, and that theſe are only agents to produce that effect upon ſomething elſe, that in all caſes, the quantity of air produced bears a certain general proportion to the capacity of the veſſel in which the proceſs is made, never, I believe exceeding one eighth, excluſive of that which is held in ſolution by the water itſelf, which, however, is pretty conſiderable.

A jar containing one hundred and fifteen ounce meaſures was filled with pump water the 2d of June, and it preſently began to yield air, and continued to do ſo about a fortnight, after which very little was produced. The quantity I received from it was twelve ounce meaſures, which is more than one tenth of the bulk of the water, and as highly dephlogiſticated as almoſt any that I had ever procured. The reaſon why this jar began to yield air immediately was, that green matter from former experiments adhered to ſeveral parts of it.

At another time I obſerved, that when a large earthen trough, filled with pump water, was very turbid, with green matter floating in it, and in a ſtate of giving air very plentifully, if I inverted any jar full of it, it would, in about a week, yield one eighth of its contents of air; and examining it, I found this air ſo pure, that with two equal quantities of nitrous air, the meaſures of the teſt were 0.5;

and it is not often that dephlogiſticated air is obtained more pure than this.

From this experiment it was very evident, that there is no proper *production* of air in the caſe, but only a *depuration* or dephlogiſtication of the air previouſly contained in the water; and as water plants depurate the air that is held in ſolution by the water, it is agreeable to analogy that plants growing in air ſhould depurate that air to which they are expoſed. This led me to try whether plants growing in air would, when wholly immerged in water, though it be not their natural element, exert, and retain for any time, their power of depurating air. But ſtill, to keep as near as I could to the water plants, with which I had had ſo much ſucceſs, I pitched upon the *water flag* for the experiment; the root of this plant and part of the ſtalk being in water; though the upper part emerges out of it. Not ſuſpecting that the mere *leaves* of a plant retain ſo much life as Dr. Ingenhouſz found them to do, (and as I might have learned from Mr. Bonnet) I took three whole plants, and put them into tall jars of water for the purpoſe; when I obſerved that the leaves were preſently covered with air bubbles, and continued to give air the whole day. This air I obſerved to ſtream from both ſides of the leaf, and every part of the ſtalk. This air I examined, and found it, in one caſe, to be ſomething worſe than common air, but in another caſe, ſomething better, though not conſiderably ſo. Before I proceeded to make trial of any other plants, I was informed of the experiments of Dr. Ingenhouſz, whoſe aſſiduous attention to this ſubject gave me the greateſt ſatisfaction, and entirely ſuperſeded what I might otherwiſe have thought of doing in the ſame way.

It appears from theſe experiments, that air combined with water is liable to be phlogiſticated by reſpiration, and to be dephlogiſticated by vegetation, as much as air in an elaſtic ſtate, out of water. For fiſhes, as I have obſerved, foul the air contained in the water in which they are confined, and water plants now appear to purify it. This is no doubt one of the great uſes of weeds, and other aquatic plants, with which freſh water lakes, and even ſeas abound, as well as their ſerving for food to a great number of fiſhes.

The experiments recited in this ſection may help us to explain why water, after iſſuing from the earth, and employed in floating meadow land, becomes in time exhauſted of its power of fertilizing them. When it iſſues from the earth, it contains air of an impure kind; that is, air loaded with phlogiſton. This principle the roots of the graſs extract from it; ſo that it is then replete with dephlogiſticated air, and conſequently the plants it afterwards comes into contact with, find nothing it in to feed upon.—I believe it is commonly imagined, that the water depoſits ſomething in its courſe upon the earth of its bed, and by that means becomes effete, and incapable of nouriſhing plants.

## SECTION III.

### *Farther obſervations on the* GREEN VEGETABLE MATTER *with which many experiments in the preceding volume were made.*

I Very much doubt whether the green matter, which had been the ſubject of the preceding experiments, has ever been properly noticed by botaniſts. The *conferva fontinalis*, as it is deſcribed by Dr. Withering, in his moſt uſeful ſyſtem of botany, though ſaid to have threads *extremely ſhort*, is only ſaid to have them "Sometimes not more than half an inch in length," and it is alſo ſaid to be of a *browniſh green*. Whereas this whole plant cannot be one tenth of an inch in length, and it is of a beautiful lively green. It will be thought, however, I imagine, to come moſt properly under the denomination of the *conferva*; but this not being within my province I ſhall not preſume to give it any particular appellation, though I might be inclined to call it *the water moſs*. I ſhall therefore continue to call it, in general, the *green matter*, or the *green vegetable ſubſtance*. Whether this plant has ever got a name in ſyſtems of botany, or not, its *natural hiſtory* is certainly unknown; and therefore, in this and the following ſection, I ſhall give ſuch an account of its mode of growth, and other particulars relating to it, as have happened to fall under my obſervation*.

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* Dr. Ingenhouſz's idea of the origin of this vegetable matter, as he himſelf allows it to be, is rather extraordinary, conſidering how long the doctrine of *equivocal*, or *ſpontaneous generation*, has been exploded. He ſays, p. 90, "The water itſelf, or ſome ſubſtance in the water, is, as "I think, changed into this vegetation, and undergoes, "by the influence of the ſun ſhining upon it, in this very "ſubſtance, or kind of plants, ſuch a metamorphoſis, as to "become what we now call dephlogiſticated air.—This "real tranſmutation, though wonderful in the eye of a "philoſopher, yet is no more extraordinary than the "change of graſs and other vegetables into fat, in the "body of a graminivorous animal, and the production "of oil from the watery juice of an olive tree." But the change of *water*, into an *organized plant*, is a thing of a very different nature from theſe.

In general the ſeeds of this plant (for I preſume that, like other plants, this alſo muſt have ſeeds) float inviſibly in the air, and are capable of producing plants in all ſeaſons of the year, whenever they meet with water, eſpecially if it be impregnated with not too great a proportion of vegetable or animal ſubſtance, in a ſtate of putrefaction; and if it does not actually freeze, the plants never fail to appear in their vigour, ſo as to be capable of producing air, in the ſpace of a few days. But though the richeſt pabulum for this plant is the putreſcent parts of animal and vegetable ſubſtances, ſome of them are unfavourable to it, and prevent its growth.

The ſeeds of this plant inſinuate themſelves into veſſels of water through the ſmalleſt apertures, and then diffuſe themſelves through the whole maſs of it; ſo that when the largeſt jars are filled with water, and placed inverted in baſons of the ſame, and conſequently the ſeeds muſt enter between the baſons and the bottoms of the jars, the plants will firſt appear at the very top of the jar, if the beſt pabulum for it be lodged there. It will likewiſe appear from the following experiments, that, though the tendency to produce pure air is favoured by a certain quantity of putrifactive matter in the water in which theſe plants are found, the quantity may be ſo great, as to counteract the operation of the plant, and phlogiſticate and diminiſh the air as faſt as it is produced.

As I ſhall generally deſcribe the whole of every proceſs, juſt as I noted the appearances at the time, the neceſſary *influence of light* in the production of dephlogiſticated air, as well as other circumſtances already proved by the experiments recited above, will occaſionally receive additional confirmation.

I have found a ſlower and a leſs produce of air from rain water than from pump water; owing, I ſuppoſe, to the rain water containing leſs air to operate upon, and generally alſo in a purer ſtate than that which is contained in pump water.

On the 8th of June, I placed, in the open air, a large jar of rain water, inverted in a baſon of the ſame; but no green matter appeared in it before the 22d of the month. On the 24th of July, finding no more air produced, I examined it, and found the quantity to be two ounce meaſures and a half: and with two equal quantities of nitrous air, the meaſures of the teſt were 1.24. This rain water, which was received in a large tub from the roof of a houſe, yielding ſo little air of itſelf, I generally made uſe of it when I tried the effect of different impregnations of water.

The green matter, and conſequently the production of air, alſo generally appeared very late in *diſtilled water*, which alſo is a confirmation of the hypotheſis mentioned above. For water after diſtillation muſt have time to imbibe air from the common atmoſphere for this plant to operate upon, before any air can be produced from it. On this account, I have always found that this effect has been ſooneſt produced in the ſmalleſt veſſels. Having, on the 20th of Auguſt expoſed to the air a jar nine inches deep, and another of four inches, not inverted in baſons, but ſimply filled with diſtilled water, the latter was covered with green matter on the 6th of September; whereas it did not appear in the taller veſſel till a conſiderable time afterwards.

In one experiment (from which, however, I would not draw any certain concluſion) diſtilled water was more favourable to the production of this green matter than rain water, which, being collected from the roof of a houſe, might contain ſome peculiar impregnation unfavourable to vegetation.

I put 4 dwt. of boiled mutton into the belly of a retort, containing about a pint, filled with diſtilled water, and an equal quantity of the ſame mutton I put into a retort of the ſame ſize, filled with rain water; and obſerving them nine days afterwards, I found the latter of a reddiſh hue, with very little air, whereas the former was all green, and in a ſtate of yielding air very copiouſly. The mouth of this retort was immerſed in a veſſel of water ſeven inches deep, and was alſo cloſed with a cork, which had a very ſmall perforation in it, in order to cut off, as much as poſſible, all communication with the external air. Perhaps the ſeeds of this plant might have been in this water previous to its expoſure, though it had been diſtilled not long before the experiment.

I found this green matter in a ſtate of giving air in water, which had formerly been impregnated with fixed air and iron. The fixed air, however, being gone, and the iron precipitated, nothing but ſimple water was left. But I likewiſe found this plant in water impregnated both with *common ſalt*, and with *ſaltpetre*, which impregnations water will not part with in the open air.

The water was impregnated with common ſalt, ſo as to make it of about the ſame degree of ſaltneſs with that of ſea water, and it was expoſed in a tube an inch in diameter, and three feet long, inverted in a pot of the ſame water. All the inſide of the tube was in time nearly covered with ſmall green knobs, almoſt contiguous to each other, and not with ſuch an uniform coating

ing as is generally found in common water. The air was very pure. Dephlogiſticated air was alſo procured at the ſame time in a ſimilar tube, filled in the ſame manner, with water impregnated with an equal quantity of *nitre*. But the air in this appeared not to be quite ſo pure as that in the water impregnated with common ſalt.

Having impregnated a quantity of water very ſtrongly with fixed air, I placed it in an inverted phial, and obſerved that no green matter appeared in it of a long time; but when the fixed air might be ſuppoſed to have made its eſcape, the green matter appeared; and the air, when examined, was found to be of the pureſt kind, without the leaſt mixture of fixed air in it. With two equal quantities of nitrous air, the meaſures of the teſt were 0.5.

In order to obſerve on what part of a veſſel of water the ſeeds of this plant would firſt fall, and in what manner they would then propagate themſelves, I placed in the ſun a glaſs tube one inch wide and three feet long, in an inclined poſition, but with its mouth upwards, filled with diſtilled water. The green matter firſt appeared, in ſmall ſpecks, about two inches below the ſurface of the water, on the ſide to which it was inclined, then on the ſame ſide near the middle of the tube, and laſtly all the bottom was covered with it. On the whole, the tube preſented the appearance of the ſeeds having been let fall into it perpendicularly, and paſſing through the water to have fixed themſelves where they happened to impinge. Had the tube been placed perpendicularly, the green matter would, I ſuppoſe, have appeared firſt at the bottom of it, as indeed I have generally found to be the caſe, and would have extended itſelf from thence to the ſides of the tube.

## SECTION IV.

### *Of the production of green matter, and of pure air, by means of various* VEGETABLE SUBSTANCES *in water.*

HAVING very ſoon obſerved that this green vegetable matter, or *water moſs*, was planted and propagated with more eaſe, and produced air more copiouſly, in ſome circumſtances than in others, and that various ſubſtances, animal and vegetable, were favourable to it, and others of both kinds unfavourable, I tried a great variety of them, and ſhall recite ſuch of the particulars as appear in any meaſure remarkable, and ſuch as may furniſh hints for the farther inveſtigation of what relates to this ſubject.

The moſt remarkable circumſtance attending theſe experiments was, that ſome ſubſtances, concerning which I could have had no ſuch expectation a priori, inſtead of admitting the growth of this plant, when they began to putrify, and diſſolve, which was the caſe with moſt vegetable and animal ſubſtances, yielded from themſelves a very great quantity of inflammable air; and it made no difference whether they were placed in the ſun or in the ſhade. Whereas other ſubſtances, which, if they had been confined by quick-ſilver, would have yielded, by putrefaction, inflammable air alſo, together with a portion of fixed air, only ſupplied the proper pabulum for this green matter, and the whole produce was pure dephlogiſticated air; the phlogiſton, which in other circumſtances would have been converted into inflammable air, now going to the nouriſhment of this plant, which by the influence of light yields ſuch pure air.

I ſhall, in the firſt place, give an account of the experiments I made with the *leaves* of plants, and then with ſome other parts of them, confining myſelf chiefly to ſuch as are commonly uſed for food; having in that choice a view to the *principle of nutrition*, beſides that ſuch ſubſtances were moſt at hand.

On the 18th of June, I put 18 dwts. of green *cabbage* into a large jar of rain water.

On the 28th the water began to be a little turbid, and the veſſel contained three ounce meaſures of air, no part of which was fixed air, and, with two equal quantities of nitrous air, the meaſures of the teſt were 1.44. Having changed the water, and left the cabbage in the ſame veſſel, on the 18th of July there was in it ſix ounce meaſures of air, which was increaſing very rapidly, all the water being very green; but after the 19th, little more air was produced. At this time I collected ten ounce meaſures, no part of which was fixed air, and with two equal quantities of nitrous air, the meaſures of the teſt were 0.67. The cabbage was then ſoft, but not offenſive.

Replacing the ſame cabbage in freſh water, on the 27th of July ſeveral ounce meaſures of air were produced, and on the 29th I took from it eight ounce meaſures, the production of air having ceaſed a day or two before. This air was quite as pure as the laſt; for the meaſures of the teſt were 0.6. and the cabbage was ſtill ſoft, but not in the leaſt offenſive. The reaſon of this, I imagine, was, that the phlogiſton, which would have conſtituted the offenſive ſmell of the cabbage (and no putrid vegetable ſubſtance is more offenſive) was, in this caſe, imbibed by this *water moſs*, as faſt as it was produced by the proceſs of putrefaction; and the veſſel being large, there was no ſuperabundant phlogiſton to contaminate the air.

In order to try what effect a larger quantity of cabbage in proportion to the ſize of the jar would have, and alſo what would be the difference of its putrefying in the *dark*, I made the following experiment.

On the 19th of July I put 2½ ounce meaſures of cabbage into a ſmall veſſel of water in the ſun, and in a ſimilar veſſel an equal quantity of the ſame cabbage in a dark room. On the 25th the water of the veſſel in the ſun had a whitiſh appearance, and about an ounce meaſure of air was produced; but at the ſame time there was a much larger quantity of air produced from the cabbage in the dark, the water being turbid alſo. The day following I examined the air from the dark room, and found it to be ſixteen ounce meaſures, one third fixed air, and the reſt ſtrongly inflammable. The cabbage was putrid and highly offenſive. That in the ſun had yielded an ounce meaſure and an half of air, a very ſmall proportion of which, perhaps one twentieth, was fixed air, and the reſt ſlightly inflammable, the cabbage offenſive.

This experiment ſhews that without light inflammable air is produced by the putrefaction of vegetable ſubſtances, and accounts for the production of this kind of air in marſhes. The reaſon why the cabbage in the ſun alſo produced inflammable air (though it was not in ſo great a quantity as from the cabbage in the dark) was that the maſs of it was too great for the capacity of the veſſel. There had alſo been very little ſunſhine, the weather having been rainy, or cloudy.

On the 28th of June I put fourteen dwt. of *lettuce* into a jar containing twenty ounce meaſures of rain water. On the third of July it became turbid, and two ounce meaſures of air were produced, the ſlighteſt proportion

portion of which was fixed air, and the reſt ſtrongly inflammable. The lettuce had a very offenſive ſmell. In this caſe, as in the former, the quantity of lettuce, as I imagined, was too great for the production of pure air.

A branch of garden *ſpurge* put into a jar of rain water, the 28th of June, had yielded but a few bubbles of air on the 17th of July, neither fixed air nor inflammable, but of the ſtandard of common air. I then replaced the ſpurge in a quantity of freſh water, and on the 27th of July I took from it an ounce meaſure and an half of air, ſo pure that, with two equal quantities of nitrous air, the meaſures of the teſt were 0.66; and it would probably have yielded more air. At the time of the firſt obſervation I imagined the plant was not ſufficiently putrid.

The green vegetable matter upon this plant was of a peculiar ſpecies, quite different from any thing that I had ever obſerved before, or have ſeen ſince. One of the berries of the ſpurge was quite covered with it, and exhibited the appearance of ſuch a figure as is generally drawn to repreſent the atmoſphere of a comet. It conſiſted of filaments as fine as a hair, each of them about half an inch in length, riſing perpendicularly from the ſurface of the berry. This beautiful appearance was firſt noticed by my friend Mr. Scholefield, who had favoured me with a viſit that ſummer. This was probably the proper *conferva fontinalis*.

The next experiment exhibits very clearly the difference between the effect of *light*, and *no light*, with reſpect to the object of this inquiry. On the 30th of July I placed half a *cucumber*, weighing 15 dwt. in a veſſel containing ſeventy ounce meaſures of water in the ſun; and on the 24th of Auguſt I took from it one ounce meaſure of air, ſo pure that, with two equal quantities of nitrous air, the meaſures of the teſt were 1.0, not in the leaſt inflammable, and without any mixture of fixed air. The cucumber was quite covered with the green vegetable matter, and had no bad ſmell.

At the ſame time the other half of the ſame cucumber, which had been kept in a veſſel of the ſame ſize in the *dark*, had yielded one third of an ounce meaſure of air, all of which was phlogiſticated, and the cucumber was very offenſive. In this caſe I doubt not that the air in its naſcent ſtate, as it may be called, was inflammable air, but had been changed into phlogiſticated air, as inflammable air is very apt to be; in which caſe the quantity is always greatly diminiſhed. Of this I ſhall produce ſeveral inſtances in the courſe of this volume.

The only *flowers* I made trial of were *white lillies*. Of theſe, on the 28th of June, I put 3 dwt. into a jar containing about forty ounce meaſures of rain water; and at one time during the proceſs they ſeemed to have yielded about an ounce meaſure of air; but on the 17th of July the quantity was manifeſtly diminiſhed, and when examined it appeared to be without any mixture of fixed air, and very nearly phlogiſticated, the meaſures of the teſt being

ing 1.7. The lillies had no bad ſmell. I doubt not but the phlogiſton, which is always exhaled in great quantities from flowers, had contributed to diminiſh and phlogiſticate the better air that had been firſt produced, though there had been but little or no appearance of green matter in this veſſel.

*Potatoes* I found to afford an excellent pabulum for this vegetable matter, and conſequently to be exceedingly favourable to the production of pure air, but ſeemingly not at all when they are boiled.

On the 24th of July, a potatoe, weighing 2 oz. 2 dwt. 12 grains, cut into thin ſlices, was put into a jar containing a hundred and fifteen ounces of rain water, and placed in the ſun. In a day or two the water became turbid, and air began to be emitted, the potatoe being quite covered with the green matter; and on the 28th all the water in the veſſel was ſo full of green matter floating in it, that nothing could be ſeen in the inſide of it. At the ſame time a low jar, containing about ſix ounces of water, with a ſmall a ſmall potatoe, not ſliced, had nearly the ſame appearance.

Afterwards, on the 3d of July, I put ſome ſlices of potatoe into a tall jar containing ſix ounce meaſures of water freſh diſtilled, having a communication with the water in the baſon in which it was inverted by a glaſs tube, with a very fine orifice in the cork with which the jar was cloſed. About the 20th of Auguſt I obſerved theſe ſlices of potatoe to be a little green, and on the 24th they were wholly ſo, the green matter firſt appearing in the baſon in which the jar ſtood, which was ſupplied from time to time with rain water.

In order to try what quantity of air I could procure by means of theſe potatoes, which appeared to be ſo well adapted to the purpoſe, I put three of them, each about the bigneſs of a ſmall walnut, into a veſſel containing 35 ounces of rain water. They yielded five ounce meaſures of air, ſo pure that, with two equal quantities of nitrous air, the meaſures of the teſt were 0.54. The potatoes were quite ſoft, but could not be ſaid to be offenſive. Again, from a ſliced potatoe weighing 2 oz. 2 dwt. 12 grains, expoſed to the ſun from the 24th of July, in a jar containing 115 ounces of rain water, I took, on the 6th of Auguſt, ten ounce meaſures of air, the meaſures of the teſt, with two equal quantities of nitrous air, being 0.58, the potatoes quite ſoft as thoſe above.

Laſtly, from 15 dwt. of *boiled potatoes*, which had been expoſed in the ſun a long time, in a ſmall receiver, I took about half an ounce meaſure of air, a ſmall proportion of which was fixed air, and the reſt phlogiſticated air. This potatoe was never green. What would have been the reſult if the quantity of water had been greater, I cannot tell.

From three ſlices of *turnip*, expoſed to the ſun in a veſſel containing ninety ounces of water, I took nine ounce meaſures of air, ſo pure that, with two equal quantities of nitrous air, the meaſures of the teſt were 0.75.

Nothing I ever tried was, in general, more unfavourable to the production of pure air than *onions*. It was only by uſing a very ſmall quantity of them, and by expoſing them to the ſun in a very large quantity of water, that I ſucceeded to make it admit the green matter. At length, however, from 5 ½ dwt. of onion, expoſed to the ſun in a jar containing 200 ounces of water, from the 6th of Auguſt to the 31ſt, I got ſix ounce meaſures of air, not in the leaſt inflammable, and ſo pure that, with two equal quantities of nitrous air, the meaſures of teſt were 1.2. At the ſame time I had expoſed 13 dwt. 23 grains of the ſame onions in a jar containing 3½ ounces of water, and on the 9th of October following I took of it a little more than half an ounce meaſure of air, all of which was phlogiſticated. It extinguiſhed a candle, and was not at all affected by nitrous air. There had been twice as much air in the veſſel a month or ſix weeks before, and then it was probably inflammable.

## SECTION V.

### *Of the production of air by means of the green matter from* ANIMAL *ſubſtances.*

ANIMAL ſubſtances were not, upon the whole, more favourable to the growth of this green vegetable matter, and the production of pure air from it, than vegetables; and different kinds of animal ſubſtances exhibited as great differences in this reſpect.

One of the firſt and moſt remarkable appearances that I had of this kind occurred in ſome experiments that I was making with *fiſhes.* It ſhews how readily the ſeeds of this aquatic vegetable find their proper pabulum, notwithſtanding a great maſs of water be in their way to it.

On the 13th of June I put three very ſmall fiſhes into a jar containing 200 ounces of rain water, inverted in a baſon of the ſame, when there was preſently a thin filmy ſubſtance peeled off from all the ſurface of the fiſhes. After this a red matter, I ſuppoſe diſſolved blood, iſſued from them, and was diffuſed through the whole maſs of water, making it very turbid. About the 23d of June the red matter became, as it were, green, the green vegetable ſubſtance adhering to it; and on the 26th the whole maſs of water was exceedingly green, and quite opake; but the denſeſt part of the green matter adhered to the fiſhes themſelves, which always ſwam on the top of the jar. I did not examine this air till the 15th of July, when I found four ounce meaſures of it, and tolerably pure, but not ſo much ſo, I am perſuaded, as I ſhould have found it ſome time before. With two equal quantities of nitrous air, the meaſures of the teſt were 1.24.

A quantity of *beef* expoſed to the ſun in a veſſel of water ſoon became green, and yielded air; but preſently the green matter, which had been diffuſed through the whole maſs of water, became yellow, or white; and from that time no more air was produced. The fleſh was putrid and offenſive. The green vegetable, I doubt not, was quite dead, through the extreme putridity of the fleſh, and the foulneſs of the water, which it had not been able to purify.

To try the difference between the effects of *light* and *darkneſs* with an animal ſubſtance, as I had done before with vegetables, on the 17th of July, I put 8 dwt. 10 grains of roaſted beef into a veſſel containing about 30 ounces of water, and placed it in the ſun, and an equal receiver, with an equal quantity of the ſame beef in a dark room. On the twentieth I perceived no appearance that ſtruck me, but on the 21ſt in the evening, I found the fleſh in the ſun quite green, and two or three ounce meaſures of air were generated; but the water in the dark room continued quite tranſparent, and in every reſpect that I could perceive, unchanged.

On the 26th the green colour of the fleſh and of the water in the ſun began to diſappear, and the veſſel had a cloudy appearance. Soon after I examined the air, and found eight ounce meaſures, very pure, the fleſh ſoft and putrid, but ſtill green on its upper ſurface. The jar which had been placed in the dark never had any air, nor was any produced from it afterwards, when it was removed into the ſun.

On the 17th of Auguſt I expoſed in the ſun, in a large retort of rain water, 3 dwts. 6 grains of roaſted beef, the neck of the retort being plunged in water nine inches deep in a jar that nearly fitted it, and moreover cloſed with a cork, in which was a very ſmall perforation, ſo as to give it as little communication with the external air as poſſible.

On the 9th of September I took from it two thirds of an ounce meaſure of air, all inflammable. The fleſh had never turned green. During the ſame time I had expoſed 8 dwts. 6 grains of the ſame beef in a jar containing 200 ounces of pump water, which had turned green and yielded dephlogiſticated air. In the former caſe the beef was more in proportion to the quantity of water, and had alſo a very obſtructed communication with the external air, from which alone the ſeeds of this green vegetable could come.

This proceſs with a ſmall quantity of *veal* was very remarkable, as this ſubſtance continued to be green, and give air, till every thing in it that could be offenſive was quite exhauſted.

On the 28th of June I put 14 dwts. of boiled veal into a large jar of rain water, and on the third of July both the upper part of the veal, and all the water, was quite green. On the fourth of July I took out half of the veal, and examining the air, I found it to be nine ounce meaſures, no part fixed air, and ſo pure that, with two equal quantities of nitrous air, the meaſures of the teſt were 0.82. The water was ſtill very green.

Part of this veal, which was then quite ſoft, I replaced in a jar of freſh water, putting the remainder of it into a ſmall jar. This never gave any air at all. But on the 18th day of July the water in the large jar was all very green, and in two days yielded five or ſix ounce meaſures of air. A little time after I examined it, and found twelve ounce meaſures, ſo pure that, with two equal quantities of nitrous air, the meaſures of the teſt were 0.57. The fleſh had no coherence and ſtill was offenſive; but on the 29th of July I took from it four ounces of air equally pure with the former, and on the ſixteenth of Auguſt half an ounce more, and then the jar had nothing offenſive in it.

The proceſs with a roaſted *tendon* of a calf's neck went on juſt as the above, with this difference, which I thought to be a little remarkable, that all the water was of a reddiſh hue before it became green, though there was no blood, or any thing red, in or about the tendon. The air which it yielded afterwards was very pure.

Perhaps the moſt ſatisfactory experiments that can be made with reſpect to the production of pure air, by means of this green vegetable ſubſtance, the pabulum that putrefaction affords it, the effect of light upon it, and again the influence of putrefaction to deſtroy that air, were ſome that I made with a *mouſe*, which I always found moſt effectual for any purpoſe in which putrefaction was required, far more ſo than pieces of ſolid meat, of any kind.

On the 21ſt of June I put a dead mouſe into a jar containing 200 ounces of water, inverted in a baſon of the ſame, which I placed in the ſun. At the ſame time I put another mouſe into a jar of the ſame ſize, filled with the ſame water, and placed it in the dark. In this veſſel the water was never diſcoloured, and very little air was produced; whereas from the mouſe in the ſun there preſently iſſued a quantity of white mucous ſubſtance, which ſoon turned to an intenſe green, and yielded air moſt copiouſly. After ſome time the whole jar was full of this thick green matter, and air roſe from every part of it; but it was deſtroyed as ſoon as it approached the upper part of the jar, where the dead mouſe floated, owing no doubt to the phlogiſtic matter which iſſued from it.

In order to verify this, I threw out the mouſe, and dividing the turbid green water into two parts, I put one half of it into a retort expoſed to the ſun, and the other into an equal retort which I placed in the dark. The water in the ſun preſently yielded permanent air, highly dephlogiſticated; whereas that in the dark gave not a ſingle bubble, but when I ſoon afterwards brought it into the ſun, it yielded air like the other.

The preceding experiments being made chiefly with the *muſcular parts* of animals, I had the curioſity to try what difference would be made with the other parts of the ſyſtem, and ſome of the ſecretions; but I was contented with a few articles under this claſs, as the extention of the experiments to all the parts of the animal ſyſtem would have been tedious, and did not ſeem to promiſe much advantage.

By means of a quantity of the *brain* of a ſheep, and alſo of the *lungs*, and of the *liver*, I procured a very conſiderable quantity of very pure air, the proceſs with each of theſe being exactly like thoſe which have been already deſcribed, and therefore not requiring to be repeated. Theſe ſubſtances immerſed in rain water were preſently covered with the green vegetable matter, which was alſo diffuſed through the whole body of the water, and the produce of air from it was very copious.

The experiments I made with *blood*, *fat*, *gall*, and *gravy* had different reſults.

Eighteen pennyweights of the craſſamentum of ſheep's *blood*, was expoſed to the ſun in a jar of rain water, containing 200 ounces; but it was always red, and never yielded more than an ounce meaſure of air, the whole of which was phlogiſticated.

No air at all was produced from a ſmall piece of *fat mutton*, expoſed in the ſame manner ten days, nor from water which had a ſmall quantity of mutton *gravy* in it.

About half an ounce of ſheep's *gall* was expoſed, together with the gall-bladder in which it was contained, on the 25th of July, in a veſſel containing 200 ounces of water, which in a few days was green, and produced air; but before the 16th of Auguſt it was almoſt all abſorbed, and ſome time after was wholly ſo. Gall, being a very putreſcent ſubſtance, might act as the mouſe in the experiment recited above; ſo that perhaps with a leſs quantity of gall, or by withdrawing it in time, I might have ſucceeded better.

It is impoſſible not to obſerve from theſe experiments, the admirable proviſion there is in nature, to prevent, or leſſen, the fatal effects of putrefaction, eſpecially in hot countries, where the rays of the ſun are the moſt direct, and the heat the moſt intenſe. For whereas animal and vegetable ſubſtances, by ſimply putrefying, would neceſſarily taint great maſſes of air, and render it wholly unfit for reſpiration, the ſame ſubſtances putrefying in water, ſupply a moſt abundant pabulum for this wonderful vegetable ſubſtance, the ſeeds of which appear to be in all places diſperſed inviſibly through the atmoſphere, and capable, at all ſeaſons of the year, of taking root, and immediately propagating themſelves to the greateſt extent. By this means, inſtead of the air being corrupted, a vaſt addition of the pureſt air is continually thrown into it.

By this means alſo ſtagnated waters are rendered much leſs offenſive and unwholeſome

ſome than they would otherwiſe be. That froth which we alſo ſee on the ſurface of ſuch waters, and which is apt to create diſguſt, generally conſiſts of the pureſt dephlogiſticated air, ſupplied by aquatic plants which always grow in the greateſt abundance, and flouriſh moſt, in water that abounds with putrid matter. When the ſun ſhines theſe plants may alſo be ſeen to emit great quantities of pure air.

Even where animal and vegetable ſubſtances putrefy in *air*, as they have ſome moiſture in them, various other plants, in the form of *mold*, &c. find a proper nutriment in them, and by converting a conſiderable part of the phlogiſtic effluvium into their own nutriment, arreſt it in its progreſs to corrupt the ſurrounding atmoſphere. So wonderfully is every part of the ſyſtem of nature formed, that good never fails to ariſe out of all the evils to which, in conſequence of general laws, moſt beneficial to the whole, it is neceſſarily ſubject. It is hardly poſſible for a perſon of a ſpeculative turn not to perceive, and admire, this moſt wonderful and excellent proviſion.

## SECTION VI.

### *Of air produced by ſubſtances putrefying in water.*

THE experiments recited in this and the following ſection were entered upon chiefly to diſcover the *principle of nutrition* in vegetable and animal ſubſtances; and they ſeem to lead us to ſuppoſe, that this principle is phlogiſton, or the principle of inflammability, in ſuch a ſtate as to be capable of becoming, by putrefaction, a true inflammable air, but not generally ſuch as to burn with exploſions, but rather with a blue and lambent flame, mixed with a certain proportion of fixed air.

In the putrefactive proceſs the phlogiſton is merely evolved, and not again combined with any thing, except what may be neceſſary to its aſſuming the form of inflammable air; but in nutrition it is immediately held in ſolution by the gaſtric juice, and in the chyle formed by it. But if any part of the aliment paſs the ſtomach, and the firſt inteſtines, without having all its phlogiſton incorporated with the chyle, that principle remains in the excrement, where it is often ſet looſe in the form of inflammable air, the ſame form that it would have taken if it had gone through the ſimple putrefactive proceſs. The phlogiſton of the aliment, thus entering into the circulation with the chyle, after anſwering purpoſes in the animal œconomy which are yet very imperfectly known to us, is thrown out again by means of the blood in the lungs, and communicated to the air, which is phlogiſticated by it.

All alimentary ſubſtances not only contain phlogiſton, but I believe are capable of yielding a proper inflammable air by putrefaction. But in the following experiments on ſuch vegetables as are generally uſed for food, *roots* ſeem to yield it in a greater abundance than other parts of plants; but there are ſome remarkable differences among them in this reſpect. For though it was ſeen in the laſt ſection, that potatoes are exceedingly favourable to the growth of that green vegetable ſubſtance, which yields pure air ſo copiouſly, owing probably to the phlogiſton they contain, *onions*, perhaps equally nutritive with potatoes, are exceedingly unfriendly to that plant; but then they yield inflammable air in an aſtoniſhing quantity, when they are left to putrefy in water. This I rather ſuſpect is a proof, that onions contain more phlogiſton, and are the more nutritive ſubſtance of the two.

On the 28th of June I expoſed to the ſun 18 dwts. of onions, in a jar of 100 ounces of river water, inverted in a baſon of the ſame. They preſently began to yield air, but without ever becoming green; and on the 15th of July the quantity was fifteen ounce meaſures, a ſmall part of which was fixed air, and the reſt ſtrongly inflammable. The water was white and turbid, and the air had a ſtrong ſmell of onions.

About the ſame time I obſerved that it made no difference, with reſpect to the quality of this air, whether the onions were placed in the light or in the dark, the principle of vegetation not being concerned in this caſe. And though I obſerved the following differences in the quantities of air produced in the ſun and in the ſhade, they were not uniform, and therefore muſt have depended upon ſome unknown accidental circumſtances.

On the 17th of July I put two onions, each weighing an ounce and a quarter, in the ſun, and two others of the ſame ſize, in a ſimilar jar in the dark. On the 23d I examined them, and had 24 ounce meaſures of air in the ſhade, and only 12 from thoſe in the ſun; but the latter was more ſtrongly inflammable than the former, which burned with more of a lambent flame, though both exploded in ſome meaſure, ſo as to be ſomething more inflammable than air from marſhes.

Having kept a quantity of this air, from the time above-mentioned to the 20th of July 1780, I found it then ſtrongly inflammable, little inferior to the inflammable air from metals. Perhaps the fixed air, which had been mixed with it before, was now completely expelled from it. It appears, however, that this kind of inflammable air has an inflammability of as permanent a nature as any whatever. The air from marſhes alſo, which, with Sig. Volta, I doubt not comes from putrefying vegetable ſubſtances, I have alſo found to be equally permanent.

On the 1ſt of Auguſt I took two halves of the ſame onion, (which was an old one, and beginning to ſprout) each half weighing 17 dwts. 12 grains, and I placed one of them in the ſun, and the other in the ſhade, both in ſimilar receivers. On the 24th of the ſame month, that in the ſun had given an ounce meaſure and three-quarters of air, of which one-fifth was fixed air, and the reſt inflammable. From that in the dark I took 2¼ ounce meaſures of air, one third of which was fixed, and the reſt inflammable. From theſe experiments I was ready to conclude, that onions (and therefore, probably, other vegetable ſubſtances) would always give more air in the dark than in the light; but the following experiments ſhewed that this is by no means the caſe always.

The 30th of July I placed in the ſun, in a veſſel containing fifty ounces of water, a part of a freſh gathered onion, weighing 9 dwts. and alſo another part of the ſame onion, and of the ſame weight, in a veſſel of the ſame ſize in the dark. On the 24th of Auguſt that in the ſun had yielded three ounce meaſures of air, all inflammable, and that in the dark had produced as nearly as poſſible the ſame quantity, and as inflammable, when the fixed air that was mixed with it was waſhed out of it. The fixed air which had been extricated in the ſun had been diſſipated by means of the free acceſs of freſh air.

Upon a former occaſion I got only fixed air from onions confined by quickſilver; but then they wanted moiſture, or were not kept till they were properly putrid. For I have ſince got inflammable air, as well as fixed air, from onions kept in quickſilver, from the 2d of September, 1779, to the 31ſt of March, 1780. The onions weighed 12 dwt. 20 gr. and the air was half an ounce meaſure, three fourths of which was fixed air, and the reſt inflammable. It appears from this, as well as many other obſervations which I ſhall have occaſion to mention hereafter, that neither fixed air, inflammable air, or nitrous air, can be produced without a conſiderable quantity of water, part of which we may therefore probably infer enters into the compoſition of theſe kinds of air; though when they *are* formed, I know no method of diſcovering, and reproducing that water.

Both *carots* and *parſnips* yield great quantities of inflammable air, and equally in the ſun or in the ſhade. I was at one time much amuſed with obſerving the inflammable air iſſuing from one of the carots in the ſun. It came ſometimes in a conſtant ſtream, or in large ſucceſſive bubbles, from one particular place, neither at the centre, nor near the outſide of the carot, but in the place where the air holes are the largeſt.

To aſcertain the quantity of air produced from a given weight of theſe two roots, I placed as much of a parſnip as, by expelling water from a cylindrical veſſel, I found to occupy the ſpace of 2¼ ounce meaſures of water, in the ſun; and the next day I took from it four ounce meaſures of air, all fixed air, the reſiduum extinguiſhing a candle. This was on the 29th of July, and on the 31ſt of the ſame month I took from it four ounce meaſures more, of which two thirds of an ounce meaſure was inflammable. On the 2d of Auguſt I again took from it four ounce meaſures, one fourth of which was inflammable, exploding with a blue flame. Laſtly, on the 24th of Auguſt, perceiving that no more air would be produced, I took from it one third of an ounce meaſure; one third of which was fixed air, and the reſt not inflammable, but phlogiſticated.

From carots occupying the ſpace of an ounce meaſure and half of water, expoſed to the ſun in rain water, from the 26th to the 31ſt of July, I took ten ounce meaſures of air, of which an ounce meaſure and half was ſtrongly inflammable exploding with a red flame; and on the 4th of Auguſt I took from them near four ounce meaſures of air, of which more than one half was inflammable. The water, which had a large ſurface, had probably abſorbed much of the fixed air. This, however, was all the air that theſe carots would yield.

An equal weight of carots, expoſed the ſame time in the dark, yielded nearly the ſame quantity of air, but only a ſmall proportion of it was inflammable. This, however, I do not attribute to the darkneſs, but but to ſome other unknown circumſtance.

A ſliced *turnip* freſh gathered, weighing near three ounces, expoſed in the ſun in rain water, yielded twelve ounce meaſures of air, one third of which was fixed air, and the reſt ſtrongly inflammable.

On the 30th of July two ounces of turnip, freſh gathered, were placed in the dark, in a veſſel containing ſeventy ounce meaſures of water; and on the 24th of Auguſt I took from it an ounce meaſure and a quarter of air, of which one ounce meaſure was phlogiſticated, not inflammable. The water was exceedingly offenſive. This phlogiſticated air had been, I doubt not, inflammable in its origin, and in much greater quantity. When a turnip was ſliced very thin, and the quantity of water large, I have obſerved before, that dephlogiſticated air was produced.

*Fruits*, I found by no means favourable to the production of pure air. Like the preceding roots, they putrefied, and yielded inflammable air, mixed with fixed air. From *peaches*, both in the ſun and in the ſhade, I got air, three fourths of which was fixed air, and the reſt inflammable; but on this occaſion the quantity of air produced in the ſun was twice as much as that produced in the ſhade; though the quantity of water in which they were expoſed was the ſame, and the peaches themſelves were, as far as I could perceive, of the ſame ſize, and in the ſame ſtate.

I placed two Morella cherries, one in the ſun, and the other in the ſhade, in equal veſſels of water. From that in the ſun I got one third of an ounce meaſure of air, and from that in the ſhade one fifth of an ounce meaſure, both inflammable. I had the ſame reſult with apricots.

Having found the capacity of theſe nutritive ſubſtances to yield inflammable air, I next tried whether they would part with any of it in *boiling*. But I found that none of them did, but only in *putrefying* afterwards; ſo that this mode of preparation (and the ſame I doubt not would be found to be the caſe with roaſting, &c.) does not deprive any of theſe aliments of any part of their nutritive power.

From 19 dwts. 18 grains of *onions* I expelled, by boiling in river water, half an ounce meaſure of air, of which one third was not abſorbed by water, and extinguiſhed a candle.

From one ounce 15 dwts. of *lettuce* I got three quarters of an ounce meaſure of air, of which half an ounce meaſure was phlogiſticated air.

From 1 oz. 16 dwts. 12 grains of *carots* I got three quarters of an ounce meaſure of air, of which about one ounce meaſure was phlogiſticated air.

Theſe differences are inconſiderable, and ſome of the air, no doubt, came from the water in which theſe ſubſtances were boiled. Afterwards the potatoes and carots, putrefying in water, yielded each more than two ounce meaſures of air, one half of which was fixed air, and the reſt inflammable. The onions yielded only about half an ounce meaſure of air, but it was of the ſame kind, and the lettuce gave only a tenth of an ounce meaſure, in which nothing could be perceived to be inflammable. But I did not begin to collect this air till a day or two after the proceſs of boiling, when I perceived ſome of the ſubſtances to be in a ſtate of yielding air.

## SECTION VII.

### *Of air produced by various ſubſtances putrefying in quickſilver.*

AT the ſame time that I was endeavouring to find what quantity, and what kind of air, various ſubſtances would yield by putrefaction in water, I was willing to aſcertain the production of air from them, and from other ſubſtances, putrefying in quick-ſilver. I find, however, that all that I thought worth regiſtering were the experiments made with ſome animal ſubſtances. A few ſimilar experiments on vegetables I have occaſionally noticed elſewhere.

By means of theſe experiments, and thoſe in the preceding ſection, it may be poſſible to determine the nutritive powers of different vegetable and animal ſubſtances, and alſo other problems in philoſophy; though too much muſt not be expected from them.

It might have been imagined, that by this means we ſhould be able to aſcertain the quantity of air that any maſs of putreſcent matter would thoroughly phlogiſticate. For any given quantity of inflammable air will completely phlogiſticate twice its bulk of common air. But it will be found that a putrefying mouſe will phlogiſticate much more than that proportion of air. There muſt, therefore, be much more phlogiſton iſſuing from a mouſe than forms the inflammable air which comes from it. Perhaps therefore that phlogiſton which contributes to animal nutrition, may alſo be more than that which enters into the compoſition of the inflammable air that comes from the putrefying ſubſtance. This is a ſubject that requires and deſerves much farther inveſtigation. I only recite the following as *leading experiments*, to the ſolution of greater problems. They are, indeed, upon too ſmall a ſcale to be of much uſe even for this purpoſe; except to ſhew that the ſame kind of ſubſtance, which in a large quantity yields inflammable air, in a ſmall quantity may yield phlogiſticated air.

A ſmall *fiſh*, weighing 1 dwt. 20 grains, being confined in quick-ſilver from the 21ſt of May to the 24th of Auguſt, gave ſomething more than half an ounce meaſure of air, two thirds of which was fixed air, and the remainder extinguiſhed a candle, but was not ſenſibly inflammable.

From 2 dwts. of well boiled *beef* I got a very ſmall quantity of air, the bulk of which was fixed air, and the reſt not inflammable. At another time, from 1 dwt. 19 grains of *raw beef*, I got 0.22 of an ounce meaſure of air, nine tenths of which was fixed air, and the reſt extinguiſhed a candle.

From 2 dwts. 5 grains of *raw lamb*, I got 0.17 of an ounce meaſure of air, the bulk of which was fixed air, and the reſt not ſenſibly inflammable: but from 2 dwts. 2 grains of well *roaſted lamb*, I got three quarters of an ounce meaſure of air, half of which was fixed air, and the reſt highly inflammable; and ſome time after I took from the ſame ſubſtance half an ounce meaſure of air more, of which three fourths was fixed air, and the reſt inflammable.

From 13 dwts. 4 grains of the *tendon* of a roaſted neck of veal, I got an ounce meaſure and half of air, of which half was fixed air, and the reſt phlogiſticated. Afterwards I took from it one ounce meaſure and three quarters of pure fixed air, with the ſmalleſt reſiduum poſſible. In the former experiment alſo, as well as on a former occaſion (mentioned vol. 3. p. 343.) I found that the inflammable air was extricated firſt, and a long time before all the fixed air was exhauſted.

Having had occaſion to make many experiments with putrefying *mice*, and having more in proſpect, I was particularly deſirous to aſcertain the quantity and quality of the air produced by a mouſe of the middle ſize putrefying in quick-ſilver, and I found as follows. A mouſe weighing 6 dwts. 3 gr. confined by quickſilver, which had putrefied from the 8th of April, had yielded on the 24th of July one ounce meaſure and three-quarters of air, of which one fourth was weakly inflammable, and the reſt fixed air. This I found, by other experiments, was nearly as much as a mouſe would yield in theſe circumſtances.

Having left another mouſe to putrefy in quick-ſilver, I took the air produced from it at different times, in order to ſatisfy myſelf more fully with reſpect to the proportion that the fixed and inflammable air bore to each other, from the beginning to the end of the proceſs. The mouſe weighed five dwts. 10 grains, and it was put into an inverted veſſel of quick-ſilver on the 13th of June. On the 26th of that month, I took from it near an ounce meaſure of air, three-fourths of which was fixed air, and the reſt inflammable, burning with a very blue flame. On the 16th of Auguſt I took from it an ounce meaſure and a quarter of air, of which four-fifths was fixed air, and the reſt, if it was inflammable at all, was ſo in the ſlighteſt degree imaginable; and laſtly, on the third of April following, I took from it a ſmall quantity of air, perhaps one-tenth of an ounce meaſure, the whole of which was, as far as I could judge, all fixed air.

When a mouſe is left to putrefy in this manner, there comes from it a great quantity of diſſolved blood, or ſome other thin reddiſh liquor. This I carefully ſeparated from what was *ſolid* in the mouſe, and found that this continued to give air, when the liquor gave little or none; ſo that perhaps it may be ſomething *ſolid* in all bodies that contributes to the formation of permanent air. By long ſtanding, however, I did get a little air from this red liquor, and it was almoſt all fixed air. It was perhaps combined with it, at its ſeparation from the mouſe.

The experiments on ſome of the different *parts* and *ſecretions* of animal bodies were made on the ſame ſmall ſcale with moſt of the preceding, and therefore they can only have the ſame imperfect uſe.

From 7 dwts. of the medullary part of a ſheep's *brain* raw, I got 4½ ounce meaſures of air, of which one fifth part of an ounce meaſure was inflammable, and the reſt fixed air. I alſo found by ſimilar experiments, that the cortical part of the ſame brain gave ſomewhat leſs air than the medullary part;

but the proportion of the inflammable to the fixed air was the ſame. No certain inference, however, can be drawn from experiments on ſo ſmall a ſcale as theſe.

Two dwts. of *mutton gravy* yielded 0.02 of an ounce meaſure of air, the greateſt part of which was fixed air, and the remainder ſeemingly inflammable.

Two dwts. of the *craſſamentum* of ſheep's blood gave only a ſmall bubble of air, too ſmall to be examined. The *ſerum* alſo yielded ſome air, the bulk of which was fixed air, and the reſt phlogiſticated.

An ounce meaſure of *milk* yielded near half an ounce meaſure of air, almoſt pure fixed air, a ſmall remainder being phlogiſticated.

An ounce meaſure and an half of the *bile* of a ſheep yielded half an ounce meaſure of air, almoſt all fixed air, the ſmall reſiduum being phlogiſticated.

I ſhould not have made theſe experiments on ſo very ſmall a ſcale, but that I expected a greater quantity of air from all the ſubſtances, and becauſe leſs quick-ſilver was wanted for the purpoſe; ſo that I could have more proceſſes going on at the ſame time. Had the ſame ſubſtances putrefied in *water*, they would have yielded many times more air.

## SECTION VIII.

### *Of the production of inflammable air from iron filings and brimſtone made into a paſte with water.*

AT the time of my laſt publication, having put a pot of iron filings and brimſtone into a jar of nitrous air (the firſt effect of which was to reduce it to one fourth of its bulk, and leave it in the ſtate of phlogiſticated air) and having ſome time after this found the air much increaſed in quantity, and ſtrongly inflammable, I had ſome doubt whether the inflammable matter came from ſome farther change in the nitrous air, or from an exhalation of proper inflammable air from the iron and brimſtone. My doubt aroſe from my never having found that this paſte of iron filings and brimſtone, whether kept in water, or in vacuo, had yielded air at any time, except in a conſiderable degree of heat. In conſequence, however, of repeated experiments, I am now ſatisfied, that the inflammable air came from this mixture. For though ſome pots of it have not yielded inflammable air, they have all, with *long keeping*, even in the temperature of the atmoſphere, yielded either phlogiſticated or inflammable air; the latter generally when the compoſition was freſh made, and the former when it was old.

These experiments have alſo led me to the obſervation, that, in this and many other caſes of the diminution of common air by phlogiſtic proceſſes, a true inflammable air is firſt produced, and in its *naſcent ſtate*, as it may be called, is immediately decompoſed, previous to the phlogiſtication of the common air. The very ſame ſubſtances which, in water or quick-ſilver, yield inflammable air, only phlogiſticate common air: ſo that I am almoſt ready to conclude univerſally, that air is never phlogiſticated, but by materials which, in certain circumſtances, would yield inflammable air; though when inflammable air is previouſly produced, and then mixed with common air, it will not be decompoſed in the temperature of the atmoſphere, except in a very ſmall degree. Theſe two kinds of air will, therefore, continue mixed without much affecting each other, except in a red heat, by which the inflammable air is fired. It is then well known to ceaſe to be a ſeparate inflammable air, the phlogiſton being ſeparated from it, and entering into the compoſition of the phlogiſticated air, into which the common air is now changed; when both the whole of the inflammable air diſappears, and likewiſe about one fourth of the common air along with it.

The experiments which led to theſe concluſions, and which I ſhall now proceed to recite, may ſerve as a caution to myſelf and others, not to be too haſty in drawing general concluſions; ſince what may appear to be the *ſame materials*, and the *ſame prepara-*

*tion* of them, may have different reſults, in conſequence of there having been ſome circumſtance, reſpecting either the materials or the proceſs, that was unnoticed, but which was the ſecret cauſe of the unexpected reſults.

That nitrous air might be changed into inflammable air, was not extremely improbable *a priori*; ſince I had found that it contained as much phlogiſton as inflammable air, bulk for bulk; and ſince it is, by ſeveral proceſſes, convertible into what has the appearance of a ſpecies of inflammable air. Beſides, in this very caſe, the ſame compoſition of iron filings and brimſtone, which I now find generally yields inflammable air in the temperature of the atmoſphere, does not do ſo at all times.

Thinking that if the iron filings and brimſtone had really yielded the inflammable air which I found in the veſſel of nitrous air, it would do the ſame in common air, I confined a large pot of this mixture in a very ſmall quantity of common air in the beginning of February 1779. But though on the 19th of May following it was increaſed in bulk, it was all mere phlogiſticated air, and had nothing inflammable in it. Even the air that was entangled within the cavities of this pot of iron filings and brimſtone, and which I catched by breaking it under water, was not inflammable. It is poſſible, however, as I obſerved before, that this phlogiſticated air might have been inflammable air in its origin, or *naſcent ſtate*, and have become phlogiſticated air afterwards. At another time I put a pot of this mixture under water, as I had done formerly, and now alſo obſerved, that though it fermented very well, and turned black, yet it did not yield a particle of air in about a fortnight: and in experiments of this kind few perſons, I believe, would look for any farther change beyond that time.

Soon after, however, I found that a pot of this mixture, freſh made, and kept under water three weeks, had yielded about its bulk of air; and this was ſtrongly inflammable. But at the ſame time another mixture of this kind, kept in the ſame circumſtances, yielded only phlogiſticated air; and yet I did not knowingly make any difference in the compoſition, always mixing equal bulks of the two ingredients.

As the phlogiſton which conſtituted the inflammable air in the experiments that occaſioned theſe muſt probably have come from the iron, and not from the ſulphur; eſpecially ſince iron alone is capable of making a very remarkable change in nitrous air, I confined a quantity of this air, in a veſſel full of iron nails, from the beginning of February to the 18th of May; but after this long interval it was only phlogiſticated air, and not in the leaſt inflammable.

Having found, however, that this mixture of iron filings and brimſtone was capable of producing inflammable air in water, I made a trial of it in quick-ſilver, and found it to have the ſame effect. For confining a quantity of this mixture in quick-ſilver from the 13th to the 30th of June, in the temperature of the atmoſphere, it had yielded, in this time, its own bulk of air, ſtrongly inflammable.

I found afterwards, in a proper number of trials, that in a ſufficient ſpace of time, this mixture increaſed all the kinds of air into which I introduced it, by the addition of a quantity of inflammable air, more or leſs, according to circumſtances known or unknown. But when the experiment was made in common air, it firſt diminiſhed it about one fourth, as I have often noted; and ſome time after that I perceived an addition made to the bulk of the air, and examining it, found it at firſt to be ſlightly inflammable, but afterwards more ſtrongly ſo. This experiment ſhews that, in the firſt inſtance, the inflammable air yielded by iron filings and brimſtone muſt have been decompoſed in phlogiſticating the common air, before it could appear in its proper form.

It appeared upon one occaſion, recited above, that one pot of this mixture, *freſh made*, produced inflammable air, at the ſame time that a pot of an *old* mixture of this kind yielded only phlogiſticated air. But at what time theſe mixtures will ceaſe to give inflammable air, and begin to yield phlogiſticated air, I cannot determine. For I find that on the 23d of June a pot of iron filings and brimſtone, which muſt have been mixed about a year before, confined in a ſmall quantity of common air, had made an addition to it of three ounce meaſures on the 26th of July; and this air was inflammable. At the ſame time I found that another quantity, which had been mixed the 1ſt of July, had yielded inflammable air, in about the ſame proportion, according to the time. Alſo ſome old iron filings and brimſtone, which had been taken out of the pot, and mixed with water the third of July, had yielded about one tenth of its bulk of air on the 2d of Auguſt, ſtrongly inflammable.

That future experimenters may form ſome idea of the quantity of inflammable air that they may generally expect from ſuch mixtures as I uſually made of iron filings and brimſtone, uſing equal bulks of of each, and therefore be leſs apt to deceive themſelves in the reſults, I ſhall recite the iſſue of ſome that I made with this and other mixtures, and which I was obliged to put an end to when I removed my habitation on the 21ſt of July, 1780.

A gallipot, containing an ounce meaſure and half of this mixture, having been confined, in a ſmall quantity of common air in the beginning of July 1779, had at the time above-mentioned produced fourteen ounce meaſures of air, ſtrongly inflammable, but the production was much more rapid at the firſt than afterwards. The mixture was very hard.

Another gallipot of the ſame ſize, put into a veſſel of water, without any air, on the 23d of June, 1779, had three ounce meaſures of inflammable air taken from it on the 26th of July following, and at this time there were eleven ounce meaſures, ſtrongly inflammable. The mixture was very ſoft.

Another equal quantity had yielded ſtrong inflammable air from the 24th of June to the 15th of July 1779, and had from that time yielded about three ounce meaſures of air, but ſlightly inflammable. The mixture was very ſoft.

There is the ſame uncertainty attending experiments made with *liver of ſulphur*, which alſo exhales phlogiſton, and produces the ſame effect both on common air and nitrous air, as iron filings and brimſtone. On the 19th of May 1779, I found a quantity of nitrous air, in which ſome liver of ſulphur had been confined from the 12th of December preceding, and which was conſiderably increaſed in bulk, to be ſtrongly inflammable; and yet another quantity of this ſubſtance, and freſh made, was confined in quick-ſilver ſeveral months without producing any air at all.

Having been led by ſome of the preceding experiments to expect, and even to believe, that common air is uſually phlogiſticated by actually decompoſing a ſmall quantity of inflammable air, admitted to it in its naſcent ſtate (notwithſtanding large quantities of inflammable air ready formed have no ſenſible effect upon it) I wiſhed to aſcertain ſo extraordinary a fact, by ſome experiments of a more deciſive nature, and with that view I made the following.

Firſt I took a pot of iron filings and brimſtone, which I had found to have been in a ſtate of yielding inflammable air in water about three months, and which I therefore preſumed would continue for ſome time in the ſame ſtate. This pot being introduced to a quantity of common air made no addition to it, but diminiſhed it, and phlogiſticated it as uſual.

I then took a quantity of this mixture, which had yielded inflammable air many months, in a veſſel of water. On the 22d of September I introduced ſome common air into the veſſel in which it was contained, and on the 26th of October I obſerved that, though this mixture, now covered with water, had thrown up bubbles of air, which mixed with the common air on the ſurface of the water, that air was ſenſibly diminiſhed, though not more than one tenth in all, and being examined was found to be phlogiſticated, and to have nothing inflammable in it. At the ſame time a quantity of dephlogiſticated air, expoſed in a ſimilar manner, was diminiſhed from 11½ to one third; and from ſrom having been very pure, the meaſures of the teſt, with two equal quantities of nitrous air, were now 1.24. A candle burned in it better than in common air, but there was nothing inflammable it.

But the moſt deciſive experiment that I made of this kind was the following. A quantity of iron filings and brimſtone were mixed, and put into a phial filled up with water, on the 24th of June 1779, and on the 25th of July following it had yielded a quantity of inflammable air, which was then all taken out; and on the 22d of September more inflammable air was produced, about two ounce meaſures in all. Having by this means ſatisfied myſelf that this mixture was in a ſtate of yielding inflammable air, I introduced to it a quantity of common air (the phial having been always kept inverted in a baſon of water) and on the 26th of October I found the common air very conſiderably diminiſhed. This air then being thrown out, the mixture was kept in the phial, now filled with water. In theſe circumſtances, it continued to yield air, and when an ounce meaſure and an half was produced, which was on the 24th of March 1780, I examined it, and found it to be ſtrongly inflammable. There could therefore be no doubt but that the common air in this experiment had been diminiſhed and phlogiſticated by an addition of inflammable air in its naſcent ſtate, or rather after it was completely, though but *newly formed*.

I do not by any means infer that, becauſe common air was diminiſhed in this caſe, in conſequence of an acceſſion of inflammable air in its naſcent ſtate, that it is never diminiſhed in any other manner; but perhaps it will be found that all the ſubſtances which we know to phlogiſticate common air are likewiſe capable of yielding inflammable air, if not in the temperature of the atmoſphere, at leaſt by means of *heat*, or in ſome other proceſs. This I found to be the caſe with metals, and it has been ſeen to be ſo, in one inſtance, with liver of ſulphur, and it is remarkably the caſe with all putrefactive ſubſtances. Vaults containing human excrements are often found to abound with inflammable air, and they, like other putrefactive ſubſtances, diminiſh common air.

The preceding experiments on the phlogiſtication of common air by means of inflammable air, led me to try whether a degree of heat ſhort of ignition would not make a quantity of inflammable air, ready formed, part with its phlogiſton to common air. For this purpoſe I mixed two thirds of common air wth one third of inflammable air, and I kept them as hot as I could do without melting the glaſs veſſel in which they were contained ſeveral hours; but afterwards they occupied the ſame dimenſions as before. This air was confined in a glaſs jar, the upper part of which was ſurrounded with hot coals, by means of the inſtrument deſcribed in my laſt vol. fig. 4.

I then tried what *length of time* would effect in this caſe; but though I always found a very ſenſible, and ſometimes a conſiderable diminution, the phlogiſtication was never completed, and the progreſs of it always ſtopped, without ever proceeding farther in any length of time afterwards. The following

ing facts in proof of this it may be juſt worth while to recite.

A mixture of one third inflammable air and two thirds common air, which had been confined by water from the 30th of June 1779, was diminiſhed the 18th of March 1780 one twelfth, and burned with a lambent blue flame.

A mixture of one third inflammable air and two thirds common air, from ſome time in the month of June 1779, was diminiſhed one thirteenth on the 20th of July 1780; but it had been diminiſhed nearly as much on the 5th of October preceding. It burned with a conſiderable exploſion.

Another quantity, one third inflammable air from marſhes, and two thirds common air, from the ſame time, was diminiſhed one tenth; but on the 18th of March preceding it was diminiſhed one twelfth, and then burned with a blue flame. At this time alſo, viz. the 20th of July 1780, it burned in the very ſame manner.

Exactly ſimilar to theſe were the experiments that I made with various mixtures of dephlogiſticated air and inflammable air. Air one half inflammable and one half dephlogiſticated, mixed in June 1779, was on the 20th of July 1780 diminſhed one fifth; but it had been diminiſhed two thirds of it the 4th of October preceding. It burned with a conſiderable exploſion.

Another mixture of the ſame kind, and made at the ſame time, was, on the 3d of July 1779, diminiſhed two elevenths, and on the 1ſt of January 1780, it was diminiſhed two fifteenths more. On the 20th of July 1780, it was in the very ſame ſtate, and burned with a very conſiderable exploſion.

I had, however, better ſucceſs when, in imitation of an experiment recited above, I admitted the inflammable air to the common air at different times, and in very ſmall quantities. This I did every day, and at the ſame time kept the veſſel containing it very near the fire. I took no exact notice of the quantity of inflammable air that I mixed with it in all, but the bulk of the common air was not diminiſhed by this addition. Nothing inflammable could be perceived in it, and yet it was ſo far phlogiſticated, that, with an equal quantity of nitrous air, the meaſures of the teſt were 1.36; when, with the ſame nitrous air, and the common air, the meaſures were 1.26.

I do not know, therefore, but that, with much patience, admitting a ſmall bubble of inflammable air every day, to a large quantity of common air, it might not be both diminiſhed in bulk, in the uſual proportion of one fourth of the whole, and completely phlogiſticated, even in the temperature of the atmoſphere. Nothing, however, but experiment, can decide in this caſe.

## SECTION IX.

### *Of the air that has been ſuppoſed to come through the pores of the ſkin, and of the effects of the* PERSPIRATION *of the body.*

I HAVE ſometimes found it neceſſary, though it is by no means agreeable to me, to correct the miſtakes of others on the ſubject of which I am treating; and I muſt appropriate this ſection to that buſineſs.

It cannot be thought extraordinary, that when it has been imagined that air is extracted from the moſt compact bodies, as gold, by means of the air pump, it ſhould be thought to iſſue from the human ſkin. It was alſo very natural to imagine, that ſince *reſpiration* injures and phlogiſticates air, the *perſpiration* of the body, ſenſible and inſenſible, ſhould do the ſame; and they who ſuppoſe that phlogiſton converts common air into fixed air, muſt of courſe imagine, that the air contiguous to the ſkin is continually undergoing this change. Dr. Ingenhouſz aſſerts the former, and Mr. Cruikſhank, after Sig. Moſcati, the latter. On both theſe ſubjects I ſhall make ſome animadverſions, and likewiſe a few experiments that I think will be deemed concluſive, on the ſubject of perſpiration, and ſufficient to confirm what I have advanced with reſpect to it in my laſt volume.

Dr. Ingenhouſz not only ſuppoſes that air is continually iſſuing from the human ſkin; but he took pains to collect it, in a conſiderable variety of circumſtances, of which he has given a particular account p. 129. This I took the liberty to tell him I had no doubt was a deception; the air that he found not having come from the *ſkin*, but from the water in which it was plunged: and both the quality of the air that he found, and the circumſtances in which he procured it, left me no doubt upon the ſubject. It was juſt that mixture of fixed air and partially phlogiſticated air, that pump water, which he recommends for the purpoſe, generally abounds with. The bubbles of air riſing and ſwelling at the ſame part of the ſkin is by no means any proof that the air came from the ſkin: for that is always the caſe with air iſſuing from water, the air bubbles never riſing within the water itſelf, but always from ſome other body immerſed in it. All the phenomena he has deſcribed may be ſeen with a piece of metal, or glaſs, plunged in water containing air, in an exhauſted receiver; in which caſe it is eaſily ſhown, that the air does not come from the pores of the metal, or of the glaſs, but from the water itſelf: for if the water contain no air, and the ſurfaces of the metal and of the glaſs be carefully wiped, that appearance cannot be produced.

He ſays that water exhauſted of its air is not proper for this experiment, becauſe it readily abſorbs all the air as faſt as it iſſues from the ſkin. But if the experiment be made in water at all, this muſt be the only unexceptionable manner of making it; and water by no means abſorbs any kind of air ſo faſt as he deſcribes this to iſſue from the ſkin, and eſpecially ſuch a kind of air as he deſcribes, a great proportion of which is air partially phlogiſticated. It requires a long time before water, in a quieſcent ſtate, will take up any ſenſible quantity of ſuch air as this. Beſides, there is nothing that we know of the human frame, that would lead any perſon to ſuſpect that air ever iſſues from the ſkin. Where are the *air veſſels* for that purpoſe? and what is their origin, or connection with other parts of the ſyſtem? The preſent ſtate of anatomy indicates nothing on this ſubject.

To ſatisfy my friend, not myſelf, I told him I would make an experiment, which I did not doubt would convince him of his miſtake in this reſpect: I did it in the following manner. I boiled a quantity of rain water, in order to expell from it all the air it might contain, and then ſat with my naked arm plunged in a veſſel filled with it, after carefully wiping off, under water, all the bubbles of air that adhered to it. But though I continued to ſit in this manner a full half hour, not a ſingle bubble of air made its appearance afterwards. I might have examined whether this water had contained any air;

beſides what it might have been ſuppoſed to have imbibed from the atmoſphere in this interval; but I neglected to do it, and am very confident it was quite unneceſſary.

After this I need not ſay any thing to my friend's ingenious obſervations on the air which he took the pains to collect from the ſkins of old and young perſons, and his laudable endeavours to remove a popular prejudice concerning the unwholeſomeneſs of the former, and the wholeſomeneſs of the latter kind of air.

Mr. Cruikſhank's experiments, if they could be depended upon, would both prove that fixed air is compoſed of common air and phlogiſton, and that the perſpiration of animal bodies, in a healthy ſtate, has the ſame effect upon air that breathing it has, viz. phlogiſticating it, and making it noxious, which is contrary to the experiments of which I gave an account in my laſt publication; by which it appears that the air under my arm-pits, and near other parts of my body, was never leſs pure than the external air. The Abbé Fontana alſo told me, that he had always found the ſame reſult in experiments made upon himſelf. But Mr. Cruikſhank ſays, (in the ſecond edition of his *Letter to Mr. Clare*, printed in Mr. Clare's Treatiſe on Abſceſſes) that, after he had confined his leg in a glaſs veſſel, ſo as to prevent all communication with the external air, lime water poured into it immediately afterwards, came out a little turbid. But this he would probably have found to be the caſe with a ſmall quantity of lime water, poured into and out of any veſſel of the ſame ſize, on account of the great ſurface of the fluid that muſt, in thoſe circumſtances, have been expoſed to the common atmoſphere; in conſequence of which it is always known to attract fixed air.

However, partly to examine this matter more thoroughly, and with a variation that I had thought of, I repeated the experiments on my own perſpiration in various ways, and they all confirmed what I advanced before, viz. that the perſpirable matter has no ſuch effect upon the air, but leaves it as wholeſome, that is, as fit for reſpiration, as ever, judging by the teſt of nitrous air, which, however, Mr. Cruikſhank does not ſay that he ever applied in this caſe.

Purſuing his ſteps, I faſtened a moiſt ox's bladder, containing about a quart of air, cloſe about my ancle, ſo that my foot, clean waſhed and warm, as his was, was expoſed to it; and I ſat near the fire, ſo as to keep my foot properly warm a full hour. After this I carefully withdrew my foot from the bladder, without changing the air: and applying the teſt of nitrous air, the air in the bladder appeared to be of the ſame degree of purity with the external air; the meaſures of the teſt, applied in the ſame manner to both, being 1.26. I alſo admitted part of this air to lime water, and obſerved that it did not make it in the ſmalleſt degree turbid.

Willing to give more time to this experiment, that the opportunity of this perſpiration phlogiſticating the air might be the greater, I once more faſtened the bladder about my foot, juſt before I went to bed, and ſlept with it all night, keeping myſelf ſufficiently warm, from eleven to half paſt ſix in the morning, when the bladder was quite dry. However, carefully moiſtening it, and eſpecially where it was faſtned to my ankle, I withdrew my foot, without changing the air, and immediately examined it. The quantity contained in the bladder was 40 ounce meaſures. It did not affect lime water, and with reſpect to purity was of the ſame ſtandard with common air; the meaſures of the teſt with the nitrous air I happened to make uſe of, being in both caſes 1.3.

I cannot therefore but ſee reaſon to conclude, as I did before, that it is only *reſpiration*, and not the *perſpiration* of the body that injures common air.

## SECTION X.

### *Obſervations on* RESPIRATION, *with a view to aſcertain the origin of the fixed air diſcovered by it.*

IT is a prevailing opinion, and, notwithſtanding the pains I have taken to explain myſelf from time to time, is by many perſons ſtill aſcribed to me, that common air, by the addition of phlogiſton, becomes fixed air. Mr. Cruickſhank, as I have obſerved, thinks that ſome experiments of his are deciſive in favour of it, and my friend Mr. Kirwan is, I find, inclined to it. As I conceive this hypotheſis to have no foundation in fact, it may not, I think, be amiſs to animadvert a little upon it.

All the experiments that I have yet heard of, that have been imagined to favour this opinion, only ſhew that there is an appearance of fixed air when common air is phlogiſticated. But this may be the caſe if any conſiderable quantity of fixed air be contained in the common atmoſphere, either properly incorporated with it, and making part of its conſtitution, or diffuſed through it. For the addition of phlogiſton, or rather its union with common air, whereby it becomes phlogiſticated air (which is quite another ſubſtance) may precipitate the fixed air, in conſequence of its having a ſtronger affinity with the baſis, whatever that be, of common air. And that fixed air is, in ſome way or other, contained in common air, is evident from its being imbibed by lime water, whenever it is expoſed to the common atmoſphere. But beſides the fixed air which is thus capable of being attracted by lime water, common air, probably at leaſt, contains a quantity that is held in a much firmer union with it. For when lime water has abſorbed all the fixed air that it can from any portion of common air, it is as fit for reſpiration as ever; and when it is phlogiſticated, at leaſt by reſpiration or putrefaction, a much greater quantity of fixed air is (at leaſt ſeemingly) precipitated from it.

It is, I imagine, this appearance of fixed air that has led ſo many perſons to ſuppoſe that it is formed by the union of phlogiſton with common air. But if it be the addition of phlogiſton that makes one part of any quantity of common air become fixed air, why does not the addition of more phlogiſton convert the whole into fixed air, which is never the caſe? For in ſimple phlogiſtication the diminution never proceeds farther than about one fourth of any given quantity of common air, and the remainder is a thing as remote from fixed air as any kind of air can be; and it is in vain to attempt, by the addition of more phlogiſton, to convert it into fixed air.

Beſides, conſidering the great diminution of common air by phlogiſtic proceſſes, there is no greater appearance of fixed air produced by reſpiration, than has been ſuppoſed to be contained in common air, and to be precipitated from it, even admitting, as I do, that the whole of the diminution is not owing to the precipitation of fixed air. Breathing into lime water ſeems to have been the principal circumſtance that has led to the miſtake which I am now animadverting upon; but few perſons are aware how ſmall a proportion of fixed air is neceſſary to make a very turbid appearance in a great quantity of lime water.

From theſe reflections on the ſubject I was led to make the following experiments; which though they diſcover new difficulties in it, may ſerve to give ſome kind of ſatisfaction with reſpect to it, and prepare the way for farther inveſtigation.

It muſt be allowed to be a curious ſubject of inquiry, to aſcertain the quantity of fixed air naturally contained in a given quantity of common air, or to trace the ſource of the fixed air which appears in ſome proceſſes for phlogiſticating common air. Now in ſome of theſe proceſſes it ſeems to be more conſiderable than in others, and in ſome I find none at all. This remarkable difference, I own, I am not at preſent able to account for. Let the following facts ſpeak for themſelves.

The diminution of air by breathing ſeems to be leſs than by putrefaction, or ſeveral other proceſſes; and though air is not completely phlogiſticated by this means (the animals dying before it quite arrives at that term) yet the diminution ſeems to be leſs even in proportion to the degree of phlogiſtication. The diminution is evidently much greater by means of putrefaction, notwithſtanding the emiſſion of permanent air from the putrefying ſubſtance, which *a priori* there is no reaſon to ſuſpect from a living body. To make the following experiments in the faireſt manner, I made uſe of quick-ſilver, rather than of water, to confine the air.

A mouſe being ſuffered to live as long as it could in a given quantity of air, confined by quick-ſilver, I let it remain two or three days afterwards; in which time there was no ſenſible diminution of the air. I then withdrew the mouſe; and admitting lime water to the air, it was diminiſhed one twenty-eighth part of its bulk. But the precipitation of the lime was not very conſiderable. Agitation in water would have produced a farther diminution, as in the following experiment.

A mouſe having breathed as long as it could in a quantity of air confined by quick-ſilver, I admitted lime water to it as ſoon as it was dead, when there was an immediate and copious precipitation of lime. After it had ſtood two days, one nineteenth of the whole quantity was abſorbed; and by agitation in water it was reduced in all one tenth. This air being examined, with an equal quantity of nitrous air, the meaſures of the teſt were 1.76. ſo that it was ſomething ſhort of being completely phlogiſticated.

Another mouſe dying in an equal jar of air, in the ſame manner, I kept it upon the quick-ſilver four days, during which time there was no abſorption of any thing; but upon water being admitted to it, one eighth of the whole quantity diſappeared; and examining the remainder by nitrous air, the meaſures of the teſt were 1.8; which, conſidering how much of the nitrous air is abſorbed by paſſing through water, may be deemed a pretty near approach to complete phlogiſtication.

At another time a full grown, but young mouſe, lived ſeven hours, in ten ounce meaſures of common air, confined by quick-ſilver. Lime water being then admitted to the air, it became turbid. But when one fifteenth of the whole was abſorbed, the remainder ſeemed to have but little fixed air in it, though the agitation in water reduced it between one fifth and one ſixth of the whole. This was the greateſt diminution that I ever found in this way.

In theſe proceſſes it is not eaſy to determine how much of the diminution is owing to the precipitation of fixed air; but ſo far is clear from theſe experiments, that let the matter to be abſorbed be what it will, the abſorption cannot take place ſo long as the air is confined by quick-ſilver, there being nothing, as we may ſuppoſe, with which the matter to be abſorbed can unite in thoſe circumſtances; though it is ready to ſeparate from the reſt of the maſs of air upon the admiſſion of water, with which it can unite. In the caſe of reſpiration therefore, that which is ſeparated from the common air ſeems to be either all fixed air, or ſome ſubſtance ſimilar to it.

In the next place, I endeavoured to aſcertain the quantity of fixed air produced by my own reſpiration in a given time; and the quantity of air that I could phlogiſticate in that time. For this purpoſe, I put a quantity of lime water into a glaſs tube, three feet long and an inch wide, filling it ſo high as that no part of it might be thrown over, when I breathed through it, by means of a ſmall glaſs tube reaching to the bottom of the large one. In this manner I breathed two minutes. Then carefully pouring out all the turbid water, and filling a phial with it, I poured into it a quantity of oil of vitriol, enough to diſlodge all the fixed air from the precipitated lime. However, leſt this ſhould not be ſufficient, I afterwards expelled all the air that I could from it by means of heat. Then, rejecting all the permanent air that came over along with the fixed air, and allowing, as well as I could, for all that might have eſcaped, without being ſeized by the lime in the water, I eſtimated the whole produce at one ounce meaſure of fixed air.

Leſt ſome miſtake ſhould ariſe from the quantity of air contained in the water itſelf I at the ſame time expelled the air by the heat of boiling water from a phial of the ſame ſize, filled with the ſame water, and an equal quantity of oil of vitriol; and I found the quantity of air expelled from it to be quite inconſiderable. In reality, I found after this proceſs, not more than a quarter of an ounce meaſure of air that was not affected by lime water. There was alſo not more than half an ounce meaſure of fixed air collected; ſo that I allowed half of the fixed air to have eſcaped the lime water, in order to make the whole equal to one ounce meaſure.

Then, in order to eſtimate the quantity of air that I could compleatly phlogiſticate by the reſpiration of two minutes, I breathed, through a glaſs ſyphon, the air contained in a receiver that held 200 ounces of water, the receiver being inverted in a trough of water. So long I found that I could breathe the air contained in this receiver with tolerable eaſe; and examining the quality of it afterwards, by means of nitrous air, I found the meaſures of the teſt to be 1.5. I repeated the experiment with the ſame event. At the ſame time uſing the ſame nitrous air and common air, the meaſures of the teſt were 1.26.

Taking this number from 2.0, the whole quantity of common air that had diſappeared was 0.74; but in the air that I had breathed the quantity that had diſappeared was 0.5; which, taken from 0.74, leaves 0.24 for the meaſure of what this reſpired air was ſhort of compleat phlogiſtication. Uſing therefore the following proportion, as 0.74 is to 2.0, ſo is 0.24 to 64.8. This I therefore conclude to be the quantity of air which I could have compleatly phlogiſticated by the reſpiration of two minutes. It amounts therefore to 32.4 ounce meaſures, or about a quart in a minute; whereas it is generally ſuppoſed that we phlogiſticate, or as it has uſually been termed, that *we conſume* a gallon of air in a minute. And if by conſuming be meant reducing the air to a ſtate in which a candle will not burn in it, the eſtimate will be pretty near the truth.

If this proceſs can be depended upon, and if the fixed air produced by reſpiration be precipitated from the common air, it may be concluded that fixed air makes about a ſixty fifth part of the maſs of common air, which is about the ſame proportion that the permanent reſiduum bears to any quantity of fixed air. For beyond that proportion, it is not poſſible to make water imbibe fixed air.

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## SECTION XI.

### *Obſervations on putrefaction, with a view to aſcertain the origin of the fixed air diſcovered by it.*

THE uncertainty of the concluſion from the experiments recited in the preceding ſection, ariſes chiefly from the quantity of fixed air that may be ſuppoſed to eſcape the lime water through which it is breathed. But I think that I made a pretty liberal allowance by ſuppoſing it to be one half of the whole, conſidering how very readily fixed air is abſorbed by quick lime in water. If, however, this one ſixty fifth part, or even more than double that quantity, be all the fixed air that is diſcoverable in common air by means of reſpiration, there muſt be ſome other cauſe of the diminution of air produced by phlogiſton, even in this proceſs, beſides the precipitation of fixed air. For in a complete phlogiſtication (for which I made the above calculation) the diminution is nearly one fourth of the whole. And when the diminution of the air is made by putrefaction, not only does it amount to a complete fourth part of the whole, notwithſtanding the production of ſome permanent air from the putrefying ſubſtance, but it has, in all reſpects, the appearance of being produced ſolely by the mere precipitation of fixed air.

The following experiments were made with a view to this very circumſtance, and they were made with as much attention as I was capable of giving to them. My reader will find an experiment of the ſame kind in my firſt volume on the ſubject of air; but though it is there very faithfully reported, yet as I was at that time but a novice in theſe proceſſes, I choſe to go over it again, taking it on a larger ſcale, and with ſome precautions which I did not then attend to.

Notwithſtanding what I had obſerved before, I had ſome ſuſpicion that the diminution of air, after the proceſs had been long continued in quick-ſilver, would not be quite ſo great as when it was made in water; and when conſequently that part of the air which had diſappeared had an opportunity of being immediately ſeparated from the reſt, and imbibed by the water, with which it was in immediate contact.

Having this circumſtance particularly in view, on the 13th of March 1780, I took two dead mice, of about equal ſize, and put them into two ſeparate cups, under different jars of common air, of very nearly equal capacities, one of them containing 155 ounces of water, ſtanding in quick-ſilver, and the other 160 ounces, ſtanding in water.

Leaving them in the country to the care of a perſon who ſupplied the veſſels in which they ſtood occaſionally with water or quick-ſilver, I went to London, and after my return, in the beginning of Auguſt, I found, by marking the veſſels, and meaſuring them afterwards, that the air in the veſſel which had ſtood in water was reduced to 140 ounce meaſures; and on the 28th of Auguſt it was reduced to 135, but after ſtanding a fortnight longer, it was not ſenſibly diminiſhed any farther. The air in the veſſel which had ſtood in quick-ſilver was not ſenſibly diminiſhed at all.

Admitting lime water to this veſſel, it preſently became turbid; but this being a ſlow diminution I removed the veſſel after ſome days to a trough of water, and then found that the air contained in it made lime water exceedingly turbid; and agitating this air in ſmall portions it was preſently reduced to 125 ounce meaſures; ſo that all the quantity diminiſhed ſeems to have been fixed air, making lime water turbid, and being abſorbed by water in the very ſame manner.

The air in the veſſel which had ſtood in water, notwithſtanding the opportunity there was for the fixed air depoſited by it being readily abſorbed, made lime water very turbid; and by agitation in ſmall portions this air was reduced to 130 ounce meaſures. Upon the whole then it appears, that the diminution in both of theſe caſes was nearly equal, viz. a little more than one fifth.

In theſe experiments the two mice were thoroughly putrefied, and indeed quite diſſolved, and no doubt had yielded all the air they were capable of yielding. But if the experiments on the putrefaction of mice in quickſilver recited above be compared with theſe, it will be found that the addition of fixed air, or air of any other kind, from the putrefied mice was quite inconſiderable, viz. an ounce meaſure and half of fixed air, and half an ounce meaſure of inflammable from each.

It is true that mice putrefying in *water* yield perhaps more fixed air than in this proportion; but here they putrefied in *air* only. And that a very inconſiderable quantity is produced in theſe circumſtances, is evident from there being little or no increaſe of the air when it is confined by *quick-ſilver*, which could not imbibe fixed air, if any had been diſcharged from the putrefying mice.

If we were to eſtimate the proportion that the fixed air naturally contained in the atmoſphere bears to its other conſtituent parts, from the data ſupplied by theſe experiments, it muſt be conſidered as not leſs than one fifth of the whole maſs; and yet it is equally certain that in other phlogiſtic proceſſes, the diminution has been quite as much without any appearance of fixed air. That there is no appearance of any when iron filings and brimſtone are made uſe of, I obſerved before, and endeavoured to account for; but I am not able to account for it in the following experiments, at leaſt in one of them.

Having, for a purpoſe that will be mentioned hereafter, introduced a quantity of nitrous air to the uſual proportion of common air, confined by quickſilver, I obſerved that the diminution was complete without any admiſſion of water; and lime water being afterwards admitted to this air was not made turbid by it. Poſſibly, however, the fixed air in this caſe might unite with the ſaline ſubſtance formed by the union of the nitrous acid and quick-ſilver; as all the ſaline ſubſtances on which I have yet made the experiments do yield ſome fixed air. But I cannot imagine what could have become of the fixed air, if there be any depoſited by phlogiſtication from the common air, in the following caſe.

Air is as capable of being phlogiſticated and diminiſhed by inflammable air, as by nitrous air; and I found the ſame proportion of it ſufficient for the purpoſe; but inflammable air muſt be ignited before it can part with its phlogiſton to common air. I made the experiment repeatedly in quick-ſilver, by means of electric exploſions, and obſerved that the whole diminution was always produced inſtantaneouſly, and even lime water admitted to the air immediately afterwards did not make it in the leaſt turbid, or produce any farther diminution. This reſult therefore was the very reverſe of the diminution of air by reſpiration, and eſpecially by putrefaction.

I not only repeated this experiment ſeveral times, and with as little loſs of time as poſſible transferred the diminiſhed air to lime water; but I made the diminution itſelf in lime water, without producing any turbid appearance whatever.

I alſo made repeated diminutions of common air by means of inflammable air and the electric ſpark over *water*, in order to diſcover what it was that the air loſt in phlogiſtication, and what becomes of that part which had diſappeared; ſuſpecting that it might have been imbibed by the water, ſo as to be capable of being reproduced in the form of air by the application of heat. But the reſult was exactly ſimilar to what I had obſerved when the diminution was made by the ſame means over quick-ſilver. For in this caſe alſo the whole of the diminution took place at once, and no fixed air was afterwards found in the water.

To make this experiment to the moſt advantage, I mixed a large quantity of air, one third inflammable, and two thirds common, and then took of it ſuch a quantity as I found by experience I could eaſily manage at one time; and putting it into one of Mr. Nairne's inflammable air piſtols, previouſly filled with water, I carefully cloſed the orifice, by tying round the mouth of it a moiſtned bladder, out of which all the air was very carefully preſſed.

When, in theſe circumſtances, the air within the piſtol was fired by means of the electric exploſion, the firſt effect was, that the expanded air was inſtantly thrown with great violence into the bladder, together with the water contained in the cavity of the piſtol; but immediately after, the air, the water, and even the greateſt part of the bladder itſelf, were forced by the external air in the piſtol; the air contained in the piſtol being now reduced by phlogiſtication into leſs ſpace than it had occupied before. I then carefully withdrew the bladder, and preſerving the ſame water, repeated the ſame experiment with it, till I had decompoſed ſo much air, that even the quantity that had diſappeared of the common air, excluſive of the inflammable air, muſt have been conſiderably more in bulk than the water. Then putting this water into a phial, I endeavoured to expel air from it by heat; but I found no more in it than ſuch water uſually contains, which was quite inconſiderable; for it was rain water which had been boiled not long before, for the purpoſe of expelling all its air. The water indeed had a turbid appearance, but this was probably occaſioned by the bladder. Had it come from the air, and eſpecially fixed air, it would have been driven out by boiling.

## SECTION XII.

### *Of changes produced in various kinds of air by the ſame proceſſes.*

THAT phlogiſton exhaling from vegetable or animal ſubſtances ſhould ſenſibly affect common air, or dephlogiſticated air, which contain little or no phlogiſton, and have a ſtrong affinity with it, is far from being extraordinary ; but that the ſame ſubſtances which phlogiſticate common air, or dephlogiſticated air, ſhould likewiſe affect nitrous air, or inflammable air, which already contain phlogiſton (and, as it ſhould ſeem, to a complete ſaturation) is a fact that I cannot well explain. This, however, I have obſerved to be the caſe with liver of ſulphur, iron filings and brimſtone, and various other ſubſtances on nitrous air.

Inflammable air I have not obſerved to have any impreſſion made upon it by theſe ſubſtances, any more than by the electric ſpark, at leaſt in the temperature of the atmoſphere; though, in conſequence of ſimple confinement

ment by water, it has at length, in ſeveral inſtances, loſt its inflammability, and, like nitrous air in the preceding circumſtances, has become mere phlogiſticated air. I have ſome ſuſpicion, however, that inflammable air may be decompoſed by all the ſame ſubſtances that decompoſe nitrous air, if more *heat*, or more *time*, be given to the proceſs; and perhaps what I conſidered as *pure water* might, in time at leaſt, have got an impregnation of ſomething that might affect the inflammable air, in thoſe caſes in which I found it reduced to the ſtate of phlogiſticated air. Theſe ſuſpicions I have been led to in conſequence of obſerving that *urine* had this effect, both on nitrous and inflammable air; an obſervation which I made accidentally, in the courſe of expoſing a great variety of ſubſtances to the ſun during the ſummer of 1779.

Among other things, I had filled a glaſs tube, about half an inch in diameter, and three feet and a half long, with urine, and had placed it inverted in a baſon of the ſame. In this ſituation it was kept ſeveral months, when it yielded at firſt a ſmall quantity of air, all of which was afterwards abſorbed. After which I perceived cryſtals to be formed in ſeveral places of the inſide of the tube, and the urine, from being of a pretty high colour, became very pale.

Seeing no farther change in the urine, and having obſerved its power of emitting and abſorbing air, I expoſed to its influence all the kinds of air that could be confined by it, in ſeparate ſix ounce phials, of which the air of each kind occupied about one fourth, the remainder of the phial containing this old pale urine; and the phials were inverted in baſons of the ſame, and, as it evaporated, were ſupplied from time to time with more urine.

Things were diſpoſed in this manner on the 27th of July, and I obſerved that there was no immediate change, either in the inflammable or the phlogiſticated air; but the ſurface of the urine in contact with the common air, dephlogiſticated air, and nitrous air, from a pale ſtraw colour, preſently became of a deep brown, and eſpecially next the dephlogiſticated air. The next morning the colour of the urine in contact with the dephlogiſticated air was almoſt black, and extended through the whole phial. But in the phial in which the common air was confined, the brown colour extended only a little way within the body of the urine. Under the nitrous air the urine was pretty uniformly brown, but not ſo much ſo as under the dephlogiſticated air. A little both of the dephlogiſticated and nitrous air was abſorbed, and nearly an equal quantity of each.

The dephlogiſticated air having diminiſhed very faſt, and becoming thoroughly phlogiſticated, I introduced more of the ſame air into the phial; and I let all the phials ſtand in this ſituation till the 22d of July following, when I was obliged to put an end to the proceſs, and I then noted the following appearances. The common air was diminiſhed about one fourth, and was thoroughly phlogiſticated; the urine being of an orange colour, but not very deep. The dephlogiſticated air, having been renewed, was not completely phlogiſticated, but was nearly ſo. The nitrous air was diminiſhed one half, had changed the urine black, extinguiſhed a candle, and did not affect common air at all, ſo that it was mere phlogiſticated air. But what is remarkable, this phlogiſticated air was in a much greater proportion than is generally procured from nitrous air. This effect I aſcribe to the *length of time* that the proceſs took up, and I ſhall hereafter produce another remarkable fact in confirmation of this opinion.

The inflammable air was diminiſhed to about one eighth of its bulk, and was ſtill ſlightly inflammable. With more time I doubt not this air would have loſt all its inflammability. The urine in this phial was of a very pale colour.

The phlogiſticated air alone remained quite unaffected during the whole proceſs, and the urine was of the ſame colour with that under the common air (viz.) a light orange; but this change probably came from that part of the urine, which had been expoſed to the common air in the cup, and had gradually extended itſelf to the urine in the phial.

If the diminution of all theſe kinds of air was owing to phlogiſton, it may be inferred that this principle in the phlogiſticated air has a firmer union with its baſe than it has in nitrous or inflammable air, being leſs capable of either receiving more, or of parting with what it has got.

But perhaps the moſt puzzling circumſtance in this proceſs is, that the diminution of both the dephlogiſticated and nitrous air ſhould be accompanied with the ſame change of colour in the urine expoſed to them. A ſimilar change of colour in a ſolution of copperas, I thought was owing to the phlogiſton depoſited from the decompoſed nitrous air. But if this was the cauſe of the ſimilar change of colour in this caſe, how came the ſame change to take place in conſequence of the diminution of dephlogiſticated air, this diminution being, no doubt, owing to its receiving phlogiſton from the urine? It can hardly be that the ſame change ſhould take place in the colour of the urine whether it contains more or leſs phlogiſton than it naturally has. Perhaps it may be ſomething common to the conſtitution both of nitrous and dephlogiſticated air, and not phlogiſton, that, when they are decompoſed, is precipitated, and produces this change of colour in the urine.

On the 17th of July 1779, I expoſed all the ſame kinds of air to a quantity of water in which I had diſſolved as much *bay ſalt*, as made it equal to the ſaltneſs of the ſea; having been led to do ſo, in conſequence of having obſerved certain appearances in water of this degree of ſaltneſs, expoſed along with the urine above mentioned to the ſun. But the reſults in this caſe were not very remarkable.

On the 28th of July I obſerved that the water of the phial in which the nitrous air was contained was become of a darker colour, and that about a tenth of the air was abſorbed. In the other phials I perceived no change at all. On the 24th of July 1780, when I put an end to the experiment, the nitrous air was diminiſhed about one half, and had loſt ſo much of its virtue, that when it was mixed with an equal quantity of common air, the meaſures of the teſt were 1.4. The dephlogiſticated air was a little diminiſhed, and altered in proportion; but the common air, the inflammable air, and the phlogiſticated air, were not ſenſibly changed at all. The reſult of this proceſs will be eaſily perceived to be exactly ſimilar to that of the former, but the *cauſe of diminution* ſeems not to have acted ſo powerfully in this caſe as in that.

That *pure water* does not injure air, either in the form of vapour, or otherwiſe, is I think ſufficiently evident from obſerving that the air within one of the glaſs tubes, hermetically ſealed, in which a quantity of water had been expoſed ſeveral months, in a ſand heat, was not at all injured by it; and the trial was made more than a year after an end had been put to the experiments with the ſand heat. Nitrous air alſo remained unchanged in the ſame circumſtances.

## SECTION XIII.

### *Of the reſpiration of fiſhes.*

I HAD formerly found that fiſhes injure the air contained in ſolution in the water in which they live, vol. III. p. 342; the water in which they had been confined appearing to contain air of a worſe quality, than it did before they were put into it. I had alſo before obſerved the effect of water impregnated with fixed air, and with nitrous air, on fiſhes put into it. I have ſince repeated all theſe experiments with an attention to more circumſtances; and they both confirm and extend my former general concluſions.

Having at hand ſome water from the Hot-well at Briſtol, which I had found to contain air in a ſtate of great purity, I completely filled a large phial with it, and I put into it a few very ſmall fiſhes, which I had provided for the purpoſe of theſe and other experiments. They were minows, and other ſmall fiſhes, about two inches in length. In this water they were confined without any acceſs of common air till they died.

After this I took equal quantities of the water in which the fiſhes had died, and of that out of which it had been taken, when they were confined in it; and I expelled from both all the air which they would yield. That from the water in which no fiſhes had been put, exceeded in quantity that from the water in which they had been confined in the proportion of three to two; and examining the quality of both theſe quantities of air, by the teſt of nitrous air, the former exceeded the latter in a ſtill greater proportion. The air from the water in which no fiſhes had been confined was about the ſtandard of common air, but that which had been contaminated by the reſpiration, as I may ſay, of the fiſhes, though not thoroughly phlogiſticated, was ſomething worſe than air in which a candle juſt goes out. I ſhould probably have found it ſtill worſe than this, if I had expelled and examined the air immediately; but the water remained in an open veſſel all night before I made the experiment upon it.

From this experiment it may be concluded with certainty, that air contained in water, in an unelaſtic ſtate, is as neceſſary to the life of fiſhes, as air in an elaſtic ſtate is to that of land animals. It is not properly *water* that receives the phlogiſton diſcharged from the fiſhes, but the *air* that is incorporated with it. And this may poſſibly be the reaſon of the attraction which, in many of my experiments, there appears to be between phlogiſton and water; whereas it has been an opinion univerſally received among chemiſts, that water has no affinity whatever with phlogiſton.

From this experiment I had no doubt, but that putting fiſhes into water impregnated with air that was thoroughly phlogiſticated, would be injurious, if not fatal to them, as much as the ſame kind of air, in an elaſtic ſtate, is to land animals; and this was verified by the following experiments; from which, however, it appears that fiſhes, like inſects, and ſome other exanguious animals, can live a conſiderable time without any thing equivalent to reſpiration.

ration. What limits that time has, may in ſome meaſure appear from theſe obſervations.

I began with water that contained, as far as we are able to diſcover, no air at all. For it was rain water, that had been recently boiled a conſiderable time. The veſſel contained about three pints of it; and into this, without admitting any air at all, I put nine of the ſmall fiſhes abovementioned, and they lived in it between three and four hours. This experiment reſembles the putting of frogs and ſerpents into a vacuum, only that there was no expanſion of air contained in them to ſwell their bodies in this caſe.

Taking the ſame water, which as I obſerved, contained little or no air, I made it imbibe as much as I could of a quantity that had been phlogiſticated with iron filings and brimſtone, ſix months before. Of this, however, the water would take but very little. Into a pint of this water, thus imperfectly impregnated, I put two of the fiſhes, and they lived in it near an hour. The reſult was the ſame when I impregnated an equal quantity of the ſame water with inflammable

ble air. For in this caſe alſo the two fiſhes lived about an hour. This experiment reſembled the putting of mice, and other land animals, into phlogiſticated or inflammable air, which is known to be fatal to them, but more ſuddenly than this water was to the fiſhes, owing, I ſuppoſe, to its imperfect impregnation.

When I impregnated water with nitrous air, on a former occaſion (ſee vol. II. p. 231) I obſerved that fiſhes put into it were immediately ſeized with convulſions, and died preſently; juſt as they did in water impregnated with fixed air. But though at that time I took all the care I could to prevent the decompoſition of the nitrous air, that remained after the operation, filling the phial in which the proceſs was made with freſh water by means of a funnel, &c. ſtill a decompoſition of ſome ſmall part of it would neceſſarily be made, before I could poſſibly ſlip the funnel into the neck of the phial. To prevent this, I now introduced the fiſhes into the veſſel in which I had impregnated the water while it remained inverted in the baſon, the remainder of the nitrous air not imbibed by the water ſtill reſting upon it. The phial I uſed contained ſomething more than a pint, and the nitrous air occupied about one fourth of it.

Into this veſſel, thus prepared, I introduced two of my ſmall fiſhes, and they continued very quiet, without being ſeized with any convulſions, ten minutes, or a quarter of an hour, before they died. The cauſe of the convulſions, therefore, in the former experiment, muſt have been not the *nitrous air*, properly ſpeaking, but the *nitrous acid*, though in ſo very ſmall a quantity, diffuſed in the water, and acting like the fixed air (which is only another kind of acid) in the water impregnated with it. Whereas in this experiment the fiſhes were no otherwiſe affected than they were in the water impregnated with phlogiſticated or inflammable air, except that the water imbibed much more of the nitrous air, and on that account was ſooner fatal to them.

## SECTION XIV.

### *Of the production and conſtitution of dephlogiſticated air.*

AT the time of my laſt publication, the readieſt method I had of procuring this dephlogiſticated air was by moiſtening minium with ſpirit of nitre, and then expoſing it to a red heat in a gun barrel. But being now in poſſeſſion of ſmall furnaces, and being a little more converſant than I then was in the common operations of chemiſtry (though I ſtill have little to boaſt of in that reſpect) I find Mr. Scheele's method of procuring it (and indeed that in which it will be found that I myſelf firſt of all procured it, but without knowing what I had got) viz. from *nitre alone*, much preferable.

For this purpoſe an earthen retort, and a reverberatory furnace (for which however one black pot or heſſian crucible inverted over another anſwers very well) are neceſſary. This is alſo the cheapeſt method I know of procuring this air. For every ounce of nitre will yield at leaſt a hundred ounce meaſures of very pure dephlogiſticated air; and the fire may be ſo regulated, that the production of air ſhall be more equable, and the proceſs more manageable than in the method I had been uſed to. I therefore cannot help recommending it to all perſons converſant in theſe experiments, and who have any thing of a reverberatory furnace; though I doubt not but that covering the retort with coals, in a common fire, would anſwer very well.

Having before aſcertained the production of dephlogiſticated air from various ſubſtances containing *vitriolic acid*, without any mixture of nitrous acid, and among others from *alum*, I have ſince made one experiment in order to aſcertain the quantity of this pure air that a given quantity of alum could be made to yield. For this purpoſe I put 1 oz. 14 dwts. of calcined alum into an earthen retort, and by means of a reverboratory furnace I extracted from it one hundred ounce meaſures of air, a ſmall part of which was fixed air, and the reſt ſo pure, that with two equal quantities of nitrous air, the meaſures of the teſt were 1.0.

The water in which this air was received was ſtrongly impregnated with vitriolic acid air. This air containing much phlogiſton, and in a ſtate in which it can be imparted to air, was, no doubt, the reaſon why the air in this caſe was not ſo pure as that which is obtained from nitre. Otherwiſe this would be the cheapeſt and beſt method of procuring dephlogiſticated air.

Collecting what remained of the alum it weighed 15 dwts. and ſtill had the taſte of alum, though not very ſtrongly. More heat would probably have expelled all the acid, and conſequently would have procured more dephlogiſticated air. Alſo had the air been quite pure, it would, no doubt, have been much more in quantity. If the weight of the alum, and of the reſiduum after this proceſs be compared, it will be found that this one hundred ounce meaſures of air occaſioned the loſs of only 19 dwts. that is, not quite an ounce of the alum, and ſince the 100 ounce meaſures of dephlogiſticated air would weigh about 2 dwts. 18 grains, the weight of the vitriolic acid air, with which the water was impregnated, may be eſtimated at 16 dwts. 6 grains.

Another time I got 60 ounce meaſures of air from an ounce of alum, which is in about the ſame proportion as in the former experiment. But it had been ſo well calcined previous to this proceſs, that ſome of the air had probably been expelled in that operation; and ſtill what remained taſted very ſenſibly of alum. This air being examined, the meaſures of the teſt, with two equal quantities of nitrous air, were 1.4. There was hardly the leaſt ſenſible quantity of fixed air produced.

It is eaſy to conceive, that till any ſubſtance be completely dephlogiſticated, it cannot yield dephlogiſticated air; and it is ſomething remarkable, that a *red colour* ſhould be the criterion of dephlogiſtication, both in the calx of iron, and of mercury. Accordingly, when mercury is diſſolved in ſpirit of nitre, the produce is pure nitrous air, not only during the ſolution itſelf, but alſo during the application of heat to the yellow concrete maſs, that is formed by the evaporation of the ſolution; and no dephlogiſticated air is produced till the red precipitate is completely formed. But the action of heat upon this red ſubſtance is always followed by the production of pure air, as much as from the precipitate per ſe.

It is alſo evident from this experiment, that the air produced in it does not come from the atmoſphere, which has been conjectured with reſpect to ſome of the proceſſes for procuring dephlogiſticated air; but muſt have been contained in the ingredients, viz. ſpirit of nitre and mercury. I have no doubt but that both of them contribute to it. That ſpirit of nitre, in a very great proportion, enters into the compoſition of dephlogiſticated air, is evident from the production of it from nitre only; and that ſome *earth* is likewiſe requiſite for the purpoſe, is hardly leſs evident, from the loſs in the weight of mercury revived from red precipitate, which I have never found to be leſs than one twentieth part. This, however, is a ſmall matter; and the concluſion from it ſeems to be, that dephlogiſticated air conſiſts of about nineteen parts acid, and a twentieth part earth. I do not ſay nitrous acid always, becauſe the vitriolic is, in many caſes, no leſs capable of furniſhing it; but of ſome acid principle common to them both.

In the rapid production of all kinds of air from earthy materials, I have frequently obſerved that there is a quantity of ſuperfluous *white matter* depoſited in the cold water in which it is received. This earth ſeems to have been held in ſolution in the air while it was hot, becauſe it was then quite tranſparent, and did not become turbid till it was cool; and this is one reaſon why I think that an earth is the proper baſis of all ſuch kinds of air. For if ſome earth be certainly held in a proper ſolution, ſo as to make a conſtituent part of the air while hot, as its tranſparency ſeems to prove, and it be only depoſited by *cold*, ſome of the earth muſt be retained by it in every degree of heat, and therefore in the temperature of the atmoſphere.

phere. And perhaps no degree of cold can deprive it of all the earth that it contains. If it ſhould, I ſhould imagine that, as nothing but the *acid principle* would remain, it would then, like any other *acid air*, become liable to be immediately abſorbed by water.

This earthy matter when incorporated in the air, I ſhould imagine to be then *the ſame thing*, from whatever ſubſtance the air had been produced, being then diveſted of every thing that was peculiar to the ſubſtance from which it had been expelled ; juſt as the *acid*, in the compoſition of dephlogiſticated air, is probably the ſame thing, whether the air had been produced from materials containing ſpirit of nitre, or oil of vitriol. If this reaſoning be true, we ſhall be in poſſeſſion of a method of obtaining a truly *primitive earth*, or an *earthy principle* common to all earths, and all metallic calces whatſoever, ſince dephlogiſticated air may, as I have ſufficiently ſhewn, be produced from them all. The following obſervations, however, may perhaps lead to a contrary concluſion, or, that earth depoſited from dephlogiſticated air produced from different materials, has not, in all reſpects, the ſame properties; though I am inclined to think that, if the trials could be made quite unexceptionably, they would favour the former hypotheſis.

Having collected ſome of the white powder diffuſed through a quantity of dephlogiſticated air, procured from minium and ſpirit of nitre, I obſerved that when it was dry, it was of a grey colour, and that it was not, at leaſt immediately, affected by ſpirit of ſalt. When it was heated in a glaſs tube, by means of the flame of a candle and a blowpipe, it fumed copiouſly, and covered the inſide of the tube with a white ſubſtance; that which was not ſublimed becoming black. When it was laid upon a red hot iron, it ſmoked very much, and became of a brown colour. But in none of its forms was it quickly affected by ſpirit of ſalt, though after twelve hours this acid did acquire an orange colour, both from the black and the brown matter.

At the ſame time I had by me a quantity of white matter which, as I believe, had been collected in a ſimilar manner, when I procured dephlogiſticated air from *red precipitate*; but having loſt the label, I cannot be abſolutely certain. This matter was perfectly white when dry, and bore a red heat without any ſenſible change, nor was it affected by ſpirit of ſalt. Willing to procure ſome of this matter, that I might be ſure was from mercury, I made a quantity of red precipitate; and having put it into a gun barrel, I urged it with as great a heat as I could excite in a common fire with a pair of double bellows; but, to my ſurprize and diſappointment, all the air came over quite tranſparent. I had before made this proceſs in ſmall glaſs retorts; but I had no ſuſpicion but that I ſhould have had the ſame reſult with the gun barrel. It is a tedious proceſs to procure much of this white matter; but when I am a little more at leiſure I may poſſibly repeat my attempts.

It has been imagined by Mr. Lavoiſier, that the calces of metals attract dephlogiſticated air from the atmoſphere, in the proceſs of calcination, and conſequently that the calces expoſed to heat only throw out the very air which they had before imbibed. This is particularly his idea of the origin of that dephlogiſticated air which is procured from precipitate per ſe, which is made by expoſing mercury a long time to a moderate heat in glaſs veſſels, from which the common air is not excluded. But there is no occaſion to ſuppoſe that *all* the materials which conſtitute dephlogiſticated air are gained from the atmoſphere. For all that mercury wants to make it capable of yielding this air, is the loſs of its own phlogiſton and the acquiſition of ſome acid principle, which appears to be common to the nitrous and vitriolic, if not to other acids alſo. This acid, therefore, it ſeems to get from the atmoſphere, at the ſame time that it parts with its own phlogiſton to it.

But ſince, according to the theory of Dr. Crawford (which I do not pretend to have ſufficiently conſidered) air, in parting with its phlogiſton, acquires the *principle of heat*, are not theſe two things, viz. *heat* and *pure acid* the ſame; which is nearly the idea of Mr. Sheele? But as this principle of heat does not, in any other caſe, appear to aſſume the form of air, and has not been found to have *weight*, which all acids, and dephlogiſticated air alſo, have; it ſeems to be more probable, that the calx in parting with this phlogiſton, takes from the air two diſtinct principles at the ſame time, viz. that of heat (if Dr. Crawford's theory be true) and this acid.

Much remains to be obſerved, and to be done, on this ſubject. In the mean time I ſhall only recite an experiment which proves that precipitate per ſe, or the true calx of mercury, is much more eaſily procured in dephlogiſticated than in common air, and probably not at all in phlogiſticated air; this air not being capable of taking any phlogiſton from mercury, without which the calx cannot be formed.

I expoſed equal quantities of the ſame quick-ſilver, in equal glaſs tubes, of about two feet and a half long, and an inch and a half in diameter, but narrower towards the top, to the ſand ſame heat, for one day, one of the tubes containing phlogiſticated, and the other dephlogiſticated air, both hermetically ſealed. In the reſult, the mercury in the tube containing the dephlogiſticated air was completely covered with a coating of precipitate per ſe; but the mercury in the other tube was not ſenſibly altered. When the proceſs had been reſumed, and continued four days, I opened the tubes, and found the dephlogiſticated air ſomething worſe than it had been, but by no means ſo much ſo as I had expected; but the plogiſticated air was not at all altered. The quantity of precipitate in the dephlogiſticated air was trifling.

I then repeated the ſame experiments with common air, but in two days no precipitate was formed. With more time there probably would have been ſome. But this was ſufficient for my purpoſe, viz. to aſcertain the difference that would be produced by different kinds of air in this proceſs, according to the quantity of phlogiſton which they contained.

In the former experiment the dephlogiſticated air was confined in the ſame tube with the mercury, but afterwards I expoſed a quantity of quick-ſilver to a ſand heat in a glaſs retort, which had a communication, by means of a glaſs tube, with a large reſervoir of dephlogiſticated air. But though I continued this proceſs ſeveral days, I did not find that conſumption of dephlogiſticated air which I expected. I had little doubt, however, but that, by an attention to theſe hints, the *precipitate per ſe* may be made in much leſs time, and with much leſs expence, than it now is.

Laſtly, I would obſerve, as it has ſome little connection with the other ſubjects in this ſection, that the dephlogiſticated air which I have mentioned, vol. IV. p. 253, as having been confined by quick-ſilver, in a phial filled with iron nails, from the 13th of April 1778, being examined on the 20th of July 1780, was not diminiſhed any farther than it is there obſerved to have been in 1779. It was alſo a little worſe than it had been when it was put into the phial, and the nails were wery clean and without ruſt.

## SECTION XV.

### *Of the reſpiration of dephlogiſticated air.*

SOME of my friends have expreſſed a little doubt about the certainty of the teſt of nitrous air, as a meaſure of the wholeſomeneſs of reſpirable air in general, and of dephlogiſticated air in particular. To this I can only ſay, that every thing I have yet obſerved leads me to depend upon the accuracy of this teſt, with reſpect to dephlogiſticated as well as common air; and, according to what I ſhould think to be the faireſt method of computation, dephlogiſticated air ſerves even longer for reſpiration than from this method of examining it I ſhould have conjectured *a priori*.

The moſt natural method, as I ſhould think, for eſtimating the purity of air, and thereby judging of the *time* that any given quantity of it would ſuffice for the purpoſe of reſpiration, would be to find the quantity of phlogiſton that is required to ſaturate it, or which comes to the ſame thing, the quantity of nitrous air that is required to bring it to the ſtate of perfectly phlogiſticated air. But a mouſe will live much longer in a given quantity of dephlogiſticated air, than in this proportion, with reſpect to common air; owing, I ſuppoſe, to the animal not throwing out equal quantities of phlogiſton in equal times, but much leſs at the laſt, when the vital powers are languid, than at the firſt.

I have a glaſs veſſel which I have made uſe of in all my experiments with mice, from the beginning of my reſearches into this ſubject, a conſiderable time before I had diſcovered nitrous air. In this veſſel, which was the top of a tall beer glaſs, and which holds about two ounce meaſures of air when a mouſe of a middle ſize is confined in it, I never knew any mouſe to live longer than half an hour, and in general they have not ſurvived twenty minutes. Suppoſing, however, the full time for a mouſe's breathing the common air contained in this veſſel to be half an hour, I ſhould not have expected that a ſpecies of air which required only *four times* as much nitrous air to ſaturate it, would ſuffice for the reſpiration of the ſame animal more than four times as long; and therefore that in this veſſel of dephlogiſticated air, which at a medium requires about that proportion of nitrous air to ſaturate it, a mouſe might live about two hours. But I believe that mice in general will live conſiderably longer in that quantity of dephlogiſticated air.

I lately put a young mouſe into that very veſſel, filled with dephlogiſticated air, ſo pure that, with two equal quantities of nitrous air, the meaſures of the teſt were, 0.55. It continued there near three full hours; and being taken out alive, the air was found to be ſo far from being phlogiſticated, that it was ſtill conſiderably better than common air; for, with an equal quantity of nitrous air, the meaſures of the teſt were 1.05. Perhaps this mouſe being languid, in conſequence of its confinement, did not phlogiſticate the air ſo faſt as it would have done had it been more vigorous; But then this may in general be expected to be the caſe with all mice, in the ſame ſituation.

My friend Dr. Ingenhouſz has announced what he thought to be a very valuable diſcovery, of the Abbe Fontana's, with reſpect to the breathing of dephlogiſticated air; and had there been no miſtake in the buſineſs, it would have been a diſcovery of the very firſt magnitude. It is a method of making dephlogiſticated air ſerve thirty times longer for reſpiration than when it is breathed in the common way, ſo that a pound of nitre would yield dephlogiſticated air ſufficient for the reſpiration of a man a whole day.

"The Abbe Fontana," he ſays, p. 46, "found that an animal breathing in either "common or dephlogiſticated air, renders it "unfit for reſpiration by communicating to "it a conſiderable proportion of fixed air, "which is generated in our body, and thrown "out by the lungs as excrementitious. This "fixed air is eaſily abſorbed by ſhaking it in "common water, but infinitely more readily "by the contact with quick lime water."

The

Then, after deſcribing a method of breathing this air, which is by introducing a ſyphon through the water into the veſſel containing air, he ſays, that the diſcovery conſiſts in uſing *lime water* inſtead of common water. "The Abbe," he ſays, p. 48, "found "that the dephlogiſticated air being, after "each reſpiration, purified again by the lime "water, will remain good about thirty times "as long as it would when breathed in the "ordinary way, and that thus the quantity "of dephlogiſticated air neceſſary for one "minute will now ſerve for breathing during half an hour, and thus the expence "will be thirty times leſs.""

This language ſuppoſes that the Abbe had not only *reaſoned* upon the caſe, but that he had alſo *verified* his reaſoning by actual experiment; becauſe it is ſaid that he *found* it to be ſo. On the contrary, I can neither find any ſuch thing in fact, nor the leaſt colour for the expectation of it in reaſoning; there being no advantage whatever in breathing dephlogiſticated air in the manner that Dr. Ingenhouſz deſcribes. And his hypotheſis concerning the nature of the injury that is done to air by reſpiration is manifeſtly erroneous. For the precipitation that is made of fixed air is nothing more than a *circumſtance* attending the reſpiration of common or dephlogiſticated air, the proper effect of that animal proceſs being, as I think I have fully demonſtrated, the *phlogiſtication* of the air; and therefore, though the precipitated fixed air be abſorbed ever ſo readily, the remaining air will be but very little the better for it. For if we were to mix much more than that proportion of fixed air with the air that we breathe, we ſhould not perceive it to be at all inconvenient to us.

It was but reaſonable, however, that the aſſertion of ſo eminent a philoſopher, and the aſſertion of a *fact*, ſhould be tried by fact. I therefore took a young mouſe, and put it into three ounce meaſures and an half of dephlogiſticated air, ſo pure that, with two equal quantities of nitrous air, the meaſures of the teſt were 0.25, confined by lime water. In theſe circumſtances the mouſe lived three hours and a half; and though it was taken out alive, it died preſently after. The air, however, was not thoroughly phlogiſticated; for, with an equal quantity of nitrous air, the meaſures of the teſt were 1.35. This, though no deciſive experiment, ſhewed that the approximation to complete phlogiſtication was nearly the ſame as in the experiment recited above, in which the air was not confined by lime water.

But to make the experiment in the moſt unexceptionable manner that I could contrive, I, in the next place, got two mice, of nearly equal ſize, and put them into exactly equal quantities, viz. about five ounce meaſures, of the ſame dephlogiſticated air (the meaſure of its purity, with two equal quantities of nitrous air, being 0.24) in nearly equal and ſimilar glaſs jars, one ſtanding in lime water, and the other in common water. Both the mice continued in this ſituation ſomething more than two hours and an half, after which the air which had been confined by lime water appeared to be reduced in the proportion of 9 to 5$\frac{1}{4}$ the meaſures of the teſt being 0.96; and the air which had not been confined by lime water was diminiſhed in the proportion of 9 to $6\frac{3}{4}$, the meaſures of the teſt being 0.98. Both the mice, though kept pretty warm, laboured alike with a difficulty of reſpiration, ſome time before I put an end to experiment. In the courſe of it I agitated the lime water a little now and then, in order to make it abſorb the fixed air the better, by admitting freſh lime water to the air that had been reſpired.

It appears from this experiment, that the air confined by lime water was both diminiſhed and phlogiſticated exactly like that which had been confined by common water, by the reſpiration of mice of equal ſize, in the ſame time. The diminution indeed was, at firſt, a ſmall matter greater in the air confined by the lime water; becauſe the common water did not imbibe the fixed air ſo readily; but this made no apparent difference with reſpect to the mice, and the next day the two portions of air were found to be as nearly as poſſible of the ſame dimenſions, and of the ſame degree of purity.

In the preceding experiments, and ſeveral others which I made about the ſame time, I found that mice would not live in dephlogiſticated air till they had completely phlogiſticated it, though they lived longer in it than, in proportion to its purity, with reſpect to common air; and for this I cannot aſſign any ſufficient reaſon. I had once imagined that this was owing to my being obliged to make the mice paſs through a quantity of water, by which the air was confined; but I put a mouſe through the ſame water into a quantity of common air, and it lived in it till it was thoroughly phlogiſticated. This may deſerve a farther inveſtigation. I ſhould have put other mice into what remained of the dephlogiſticated air.

## SECTION XVI.

### *Obſervations relating to fixed air.*

MOST ſaline ſubſtances, I believe, contain more or leſs fixed air; and it may be worth while to examine what *quantity* of it may be extracted from each of them, and alſo the quality of the reſiduum, which I find to differ conſiderably in different caſes. But this may depend, in a great meaſure, upon the ſtate of the water in which the experiments are made, which will take more or leſs of phlogiſton from ſuch reſiduums. A few obſervations that I have had occaſion to make of this kind may be juſt worth noticing.

Both *vitriolated tartar*, and *Glauber ſalt*, which I have often occaſion to make in the courſe of my experiments, I find contain fixed air. Diſſolving a quantity of vitriolated tarter, which was formed in making ſpirit of nitre, and collecting the air that came from it, I found one twelfth of it to be fixed air; and with an equal quantity of nitrous air, the meaſures of the teſt for the remainder were 1.3. At another time I filled the retort in which the ſalt was contained with boiled pump water, and then I found no fixed air in it; having, I ſuppoſe, been abſorbed by the water, and the meaſures of the teſt for the remainder were 1.46. Again I diſſolved a quantity of this ſalt in pump water, and then found one fourth of the whole to be fixed air; the pump water itſelf containing a good deal, and the meaſures for the reſiduum were 1.44.

I alſo diſſolved a quantity of Glauber ſalt, which remained from the proceſs for making ſpirit of ſalt, and I found the reſiduum of the fixed air to be ſenſibly worſe than common air.

In diſſolving alum, in order to get ſome earth of alum, I obſerved that air was diſcharged from it. This I collected, and found it to contain very little fixed air, and the meaſures of the teſt for the reſiduum were 1.12. At another time I had the ſame reſult, but the air was not quite ſo good, though purer than common air.

Precipitating a ſolution of alum with pot aſh, I caught the fixed air, which was diſcharged in great abundance; and examining the reſiduum, found it to be better than common air, in the proportion of 1.2 to 1.3; the diminution being in that proportion when mixed with equal quantities of nitrous air.

## SECTION XVII.

### *Of the ſtate of air in water.*

I HAVE formerly obſerved that the ſtate of air in water is an object worthy of attention, and that it was not before the time of my laſt publication that I had ever found air extracted from water to be ſo good as common air. In general it was in part fixed air, with a reſiduum that extinguiſhed a candle. I have ſince, however, frequently found air expelled from water to be much better than common air; but I have not yet undertaken any regular courſe of experiments on the ſubject; ſuch as examining the ſame water at different times of the year, with different impregnations, different expoſures, &c. which I wiſh to have done; becauſe I think it poſſible, that ſomething worth knowing relating to the properties of water, or of air in water, eſpecially reſpecting phlogiſton, and the general ſtate of the atmoſphere, may be diſcovered by this means. Such obſervations as I have occaſionally made I ſhall here put down.

Boiling always expels more or leſs of fixed air from water. On the 5th of June 1779, I found my pump water to yield air, one fifth of which was fixed air, and the meaſures of the teſt for the reſiduum were 1.5. The ſame pump water, which had been boiled ſome time before, gave air, one ſeventh of which was fixed air, and the meaſures of the teſt for the reſiduum were 1.4. In general I believe a greater difference than this will be found in theſe two caſes. I do not know that water will attract fixed air from the atmoſphere, at leaſt in the proportion in which it is generally found in pump water, which is probably acquired from calcareous matters firſt held in ſolution, and then partially decompoſed in it.

Water diſtilled in glaſs, which had been long expoſed to the open air, yielded air, of which little or none was fixed air, and with equal quantities of nitrous air, the meaſures of the teſt were 1.1.

A quantity of rain water taken from a large tub, which had long ſtood expoſed to the open air yielded one ſixtieth of its bulk of air, of which no part was fixed air, and the meaſures of the teſt were 1.4. Perhaps the wood of the tub, or ſome other matter caſually falling into it, might have contaminated this air.

A quantity of river water, not very far from the ſpring, gave one fiftieth of its bulk of air of which the ſmalleſt part imaginable was fixed air, and the meaſures for the reſt were 1.05. This air was very pure; but the part of the river from which I took it was nearly ſtagnant, and very full of water plants.

Lime water is certain not to contain any fixed air. From a quantity of this water I expelled air ſo pure that the meaſures of the teſt were 1.0. The quantity of air was one fiftieth of its bulk. Upon the whole I am inclined to infer, from all the obſervations I have hitherto made, that this is about the ſtandard of air contained in water, which has no fixed air, and has been expoſed to no influences except thoſe of the common atmoſphere, in its uſual ſtate. But I propoſe to make more obſervations on this ſubject.

From a ſpring which was remarkable for its petrefying quality, I expected much fixed air, but I found none; and the air I extracted from it was a little worſe than common air. It is plain that, in this caſe, a boiling heat had not decompoſed the lime ſtone it contained.

I alſo filled a phial with pump water and pounded lime ſtone, expoſed to the ſun from the 28th of May to the 3d of July, when it yielded air ſo pure, that with two equal quantities of nitrous air, the meaſures of the teſt were 1.4. I ſhould have ſuſpected ſome green vegetable matter in this water, but I could not perceive any. Perhaps ſome latent, or naſcent vegetation might be the cauſe of this very pure air.

That water parts with its phlogiſton to air, and thereby becomes, in ſome meaſure, purified, independent of any thing vegetating in it, is I think evident from the following obſervation. I took ſome of the Briſtol water in which fiſhes had died, and which then yielded air thoroughly phlogiſticated; and having expoſed it to the ſun from the 28th of May to the 3d of July, I found it to yield a conſiderable quantity of air; and ſo pure that, with an equal quantity of nitrous air, the meaſures of the teſt were 0.76, and with two equal quantities of nitrous air the meaſures were 1.18.

## SECTION XVIII.

### *Obſervations relating to the conſtitution of nitrous air.*

THE ſubject of nitrous air has made a conſiderable article in all my former publications relating to air. I ſhall be obliged to appropriate ſeveral ſections to the farther obſervations that I have made upon it in this volume, and there are many things that I ſtill propoſe to inveſtigate concerning it. I ſhall begin with ſuch experiments and obſervations as more immediately relate to the *conſtitution* of this kind of air.

### 1. *Of water in the compoſition of nitrous air.*

That water enters into the compoſition of nitrous air, is not improbable becauſe it is procured in ſo great abundance from pure water impregnated with phlogiſticated nitrous vapour, and alſo from its not being procured from copper, and other metals, except in a very diluted ſolution of the nitrous acid. But notwithſtanding this I have not yet been able to diſcover any water in the decompoſition of nitrous air. The trickling of liquid ſpirit of nitre down the ſides of a glaſs veſſel in which a mixture of nitrous and common air, and more eſpecially of dephlogiſticated air is made, over water, is only the moiſture, which adhered to the glaſs, now impregnated with nitrous acid vapour, from the decompoſed nitrous air.

When I mix the two kinds of air in quick-ſilver, I cannot perceive any moiſture at all; ſo that if there be any water in the compoſition of this air, it muſt enter into the compoſition of the *mercurial nitre* formed at this time; and even in this caſe it might be expected to be diſcovered before the mercurial nitre was completely formed, which is a conſiderable time, when the ſurface of the mercury is not large.

### 2. *Of the firſt and ſubſequent produces of nitrous air.*

In ſeveral methods of producing nitrous air the quantity yielded at firſt is very ſmall, though the ſolution of the metal from which it is procured is at the ſame time very rapid. I had therefore ſome times ſuſpected, that the firſt produce, even from copper or ſilver, might differ in ſome reſpects from that which came afterwards, as I have obſerved to be the caſe very remarkably when this air is procured from tin and zinc. But I tried with copper formerly, and with ſilver lately, and in both caſes found that the firſt ſlow produce of air diminiſhed common air juſt as much as that which was produced the moſt rapidly afterwards.

### 3. *Of the changes in nitrous air when it is produced from iron.*

When nitrous air is produced from iron, the quality of it, I doubt not, is always the ſame, though there is a caſe in which I do not wonder that ſome perſons have been deceived; having got phlogiſticated air when they expected nitrous; not conſidering, that expoſure to a large ſurface of iron decompoſes nitrous air, as my former experiments ſhew; changing at firſt into a ſpecies of air in which a candle will burn, and then into phlogiſticated air. This proceſs, however, required a conſiderable time; but the following experiment ſhews that this effect may be produced very ſoon.

I filled an eight ounce phial with ſmall nails, then with water, into which I put a very ſmall quantity of nitrous acid, juſt enough to make it produce air; and then ſome was yielded in which a candle went out. Alſo this acid water poured off from the iron gave a conſiderable quantity of air, in the heat of boiling water, and this was all phlogiſticated air.

Uſing more ſpirit of nitre, I obſerved, that, though the production of air was pretty copious, as was manifeſt by the bubbles formed at the bottom of the phial, and riſing to the top, there was no increaſe of the whole quantity of air in the phial. Examining the air in this ſtate of the proceſs, I found that it had very little power of diminiſhing common air, and that a candle burned in it with a vivid flame, which is the intermediate ſtate of this air, before it becomes phlogiſticated air. I imagine, therefore, that in all theſe caſes, a proper nitrous air is firſt produced, and that it is afterwards, by means of the iron to which it is expoſed, changed into that ſpecies of air in which a candle can burn, and laſtly into phlogiſticated air, which extinguiſhes a candle.

### 4. *Of changes in the colour of liquids by which nitrous air is confined.*

I have formerly obſerved, that a ſolution of green vitriol becomes black by the contact of nitrous air. This I find to be a criterion of the preſence of a very ſmall quantity of the martial ſalts in water. When a very little of it has been accidently formed, in the courſe of my experiments, and mixed with the water in my trough, I have never failed to diſcover it by the dark colour of the water in thoſe jars which contained nitrous air.

This change of colour muſt, as I obſerved before, have been produced by the phlogiſton of the nitrous air, the nitrous *acid* having no ſuch effect. This was alſo the caſe with a ſolution of copper in nitrous acid; but the change was not from blue to a darker colour, but into *green*.

I filled a jar, about an inch in diameter, and twelve inches long, with that ſolution of copper in ſpirit of nitre which remains after making nitrous air, and which is of a beautiful blue colour. Then inverting it in a baſon of the ſame, I introduced to it a quantity of nitrous air. After ſome time I obſerved that the air was conſiderably diminiſhed, and that all the ſurface of the liquid in contact with the air, to the depth of about a quarter of an inch, was of a beautiful green colour. This air kept diminiſhing ſome months, and the green colour of the ſolution extended two or three inches within the liquid. At laſt there remained only two ſevenths of the original quantity of air; and, examining it in that reduced ſtate, I found it to be mere phlogiſticated air. Had it been examined in the intermediate ſtate, it would, I doubt not, have been found to be of that kind of air in which a candle will burn. The experiment was begun on the 4th of October 1779, and the air was not examined till the 20th of July 1780.

### 5. *Nitrous air not changed by expoſure to water in a ſand heat.*

I have obſerved that nitrous air, confined in flint glaſs tubes hermetically ſealed, was not changed by being expoſed even to a red heat, or kept ever ſo long in hot ſand, though inflammable air undergoes a remarkable change in thoſe circumſtances. I have ſince obſerved that there is no change produced in nitrous air by its being confined along with *water* in this manner. For one of the tubes which had been expoſed to the ſand heat ſeveral months, as related in my laſt publications, had both water and nitrous air in it. This nitrous air I examined the 24th of November 1779, which was a long time after its being taken out of the ſand heat; but it did not appear to have loſt any of its power of diminiſhing common air.

## 6. *Of the change in nitrous air from very long keeping in water.*

I have obſerved that if nitrous air be agitated in water preſently after it is made, or indeed after it has been kept ſome weeks, it will be reduced to a very ſmall quantity, perhaps one twentieth of its original bulk; and it will then be wholeſome air. But I find that if it be kept a very *long time*, its conſtituent principles, as we may ſay, acquire a much firmer conſiſtence, and that then a remarkably greater proportion of it becomes firſt phlogiſticated air, and then, by agitation in water, wholeſome air.

In vol. iv, p. 62, I obſerved the changes produced in two quarts of nitrous air, one from *iron*, and the other from *copper*, made in November 1773. On removing from Wiltſhire to Birmingham, I thought proper to put an end to this proceſs, when I found no farther change in the bulk of theſe two quantities of air; that which had been produced from iron ſtill occupying two thirds of its original dimenſions, and that from copper one half. I agitated in water a portion of each of theſe quantities of air, without producing any change in their bulk; but they were both conſiderably improved by it, ſo that when mixed with equal quantities of freſh nitrous air, the meaſures of the teſt were 1.75.

This I conſider as a pretty remarkable obſervation, as it exhibits a change in the conſtitution of a body depending upon *time* only, and which it is not yet in our power to produce by any other agent or inſtrument.

## SECTION XIX.

### Of the mixture of nitrous and common air.

THE buſineſs of this ſection ſhall be to explain a phenomenon which puzzled me at the time of my laſt publication in mixing nitrous and common air, and to correct a miſtake of the Abbe Fontana, and of Dr. Ingenhouſz, relating to the ſame ſubject.

I had obſerved, vol. iv, p. 77, that if, after having mixed equal quantities of the two kinds of air in my wide jar, I made them aſcend very quickly into the long tube on which the meaſures were marked, the diminution was more than when I made them aſcend more ſlowly; and this difference ſometimes amounted to five hundred parts of a meaſure. This I now conclude aroſe from what remained of the nitrous air, not decompoſed in the mixture, being diminiſhed by paſſing through ſo much ſpace of water, which is more expoſed to its influence in a ſlow than in quick paſſage. But I own I ſhould not have ſuſpected that nitrous air would have been diminiſhed ſo very much by being ſimply poured from one veſſel of water into another, if I had not obſerved it in the following manner.

Having mixed a quantity of air, which I knew to be thoroughly phlogiſticated by the putrefaction of fiſhes, with an equal quantity of nitrous air, I transferred the mixture into my graduated tube; when, inſtead of occupying two whole meaſures, as I had expected, they only occupied 1.95 meaſures. Suſpecting that the five hundred parts of a meaſure which had diſappeared had been abſorbed by the water, I poured the air back again into the wide jar; and transferring it once more into the graduated tube, found it to be only 1.8 meaſures; and pouring it about ten times backwards and forwards, without any unneceſſary agitation, it was reduced to 1.6. Having ſtood in water all night, I meaſured it again the next morning, when I found it to be 1.5; and by meaſuring three times more it was reduced to 1.4.

I then

I then poured two meaſures of nitrous air only from the wide jar into the graduated tube, and found that it was diminiſhed even in a greater proportion than the former mixture.

In applying the teſt of nitrous air, I have lately preferred equal meaſures of nitrous and of common air, or of any air which may be conjectured *a priori* to be nearly in the ſtate of common air, in order that there might be phlogiſton enough to ſaturate it entirely; and if the remaining nitrous air was not affected by water, this method would be perfectly unexceptionable; and with due precaution, it is not liable to much objection. But the moſt accurate method would be to uſe no more nitrous air than the air to be examined is able completely to decompoſe. But then it cannot be known before hand how much this is. Perhaps, in order to guard againſt the inconvenience above mentioned, it might be moſt adviſeable, in common caſes, that is, when the air to be examined is about the ſtandard of common air, to uſe ſomething leſs than an equal quantity of nitrous

trous air, but more than one half, which was the quantity that I firſt confined myſelf to.

I would farther obſerve, that if the ſimple transferring of nitrous air from one veſſel of water to another be liable to ſome uncertainty, the agitation of it, which the Abbe Fontana uſes, muſt be liable to more. For no perſon can be quite ſure that he makes the agitation exactly alike, eſpecially after any conſiderable interval of time.

No perſon has taken more pains in reducing to certainty the method of aſcertaining the purity of air by means of nitrous air than the Abbe Fontana; who has likewiſe given very great attention to the ſubject of air in general. Dr. Ingenhouſz has had his leave to publiſh a very particular account of his methods and precautions in this buſineſs; and he reaſons at large upon the advantage of each part of the proceſs.

Among other advantages of the Abbe's method, and no doubt a very capital one it would be, if he had fallen into no miſtake reſpecting it, is, that it is of no conſequence whether the nitrous air he employs be good or bad; that is, have more or leſs power of diminiſhing common air, or, which comes to the ſame thing, how much mere phlogiſticated air be mixed with it.

"In the method adopted by other philoſo-"phers," he ſays, p. 173, "by which a certain "proportion of nitrous air is always added at "once to a certain quantity of the air under "examination, the reſult is very uncertain, "if the nitrous air be not always exactly of "the ſame quality. But in the method of the "Abbe Fontana, this article is of no conſe-"quence at all. The only difference ariſing "from weak nitrous air in this method, is, "that more meaſures of it are required be-"fore the ſaturation of the air to be exa-"mined is compleated."

The circumſtance in the Abbe's method on which this pretenſion is founded, is, that, after putting a certain quantity of nitrous air to the reſpirable air, he continues to add other equal quantities, till he finds that no farther diminution is produced in the whole; ſo that, though there might not be phlogiſton enough in the firſt quantity of nitrous air, there would be in the ſecond, or third, &c. Then he deducts from the number of all the meaſures that he had put together, conſiſting of both reſpirable and nitrous air, what he finds remaining in his veſſel, and by this number he judges of the purity of the air; a greater number being an argument of greater purity in the air that he had examined. Becauſe had the mixture ſuffered no diminution at all, it muſt have been perfectly phlogiſticated.

Having no ſuſpicion but that ſo able a philoſopher, and one who had given ſuch particular attention to this very buſineſs, muſt have been aware of any conſiderable fallacy to which it had been ſubject, I was not a little rejoiced at the diſcovery. For certainly the very different power of diminiſhing common air in different quantities of nitrous air is one of the greateſt difficulties we have to contend with, in uſing it as a teſt of the purity of air. This ſatisfaction therefore I enjoyed a conſiderable time. But at length ſeeing ſome reaſon (I have now forgot what it was) to entertain a doubt about it, I firſt ſubmitted it to trial; when I preſently found that, unlikely as it had appeared to me, both the Abbe Fontana, and our common friend, had certainly been guilty of ſome great overſight; the different ſtate or quality of the nitrous air cauſing juſt the ſame uncertainty in his method of applying it, as in mine. I then, but not before, examined what my friend had advanced in praiſe of this method, when I alſo perceived a fallacy in his *reaſoning* on the ſubject.

As it is always of importance to correct the miſtakes of thoſe who will not readily be thought liable to make miſtakes, I ſhall firſt recite the *facts* that I have obſerved, and ſhall then argue from the nature of the thing; but in both I ſhall be as brief as poſſible.

I had a quantity of nitrous air, which, by long ſtanding in water, was much impaired in its virtue, ſo that, with equal quantities of common air, the meaſures of the teſt were 1.42; when with freſh made nitrous air, they were 1.29. Then, to apply the Abbe Fontana's method, by which

I was to expect that this difference would vaniſh, I mixed two meaſures of the old nitrous air with one of common air, and put them into a graduated tube, of which thirteen diviſions were equal to one meaſure. I did the ſame with the freſh made nitrous air. The reſult was, that the common air and the old nitrous air occupied the ſpace of thirty-two of theſe meaſures, while the mixture of common air and freſh made nitrous air occupied the ſpace of thirty of them; ſo that, notwithſtanding there was phlogiſton enough in each of the quantities of nitrous air completely to ſaturate the common air mixed with it, the difference in the laſt dimenſions was two diviſions. And it is plain that there muſt be the ſame difference made by deducting theſe numbers from any equal numbers whatever; as from 39, which is three times thirteen. For one of them will be 7 and the other 9, which are the quantities that have diſappeared in the proceſs.

When I uſed four meaſures of each of the kinds of nitrous air, and one of the common air, there was ſtill the ſame difference in the reſults. For ſince there could be no farther diminution produced by theſe additions, it was only adding *equal things to unequal things*, which could never tend to bring them to an equality. I repeated the experiments many times, and always had the ſame reſult. I alſo repeated them, and let the mixtures remain all night before I applied the meaſure, and I likewiſe uſed dephlogiſticated air as well as common air; but in all the trials the difference in the nitrous air made the ſame difference in the reſult, both in the Abbe's method of applying it, and in mine.

I ſhall now conſider Dr. Ingenhouſz's *reaſoning* on this ſubject; but, to be as ſhort as poſſible, ſhall only recite his application of it to the example that he produces. " Let us ſuppoſe, ſays he, p. 175, " that after " three meaſures of ſtrong nitrous air are let " up, and the ſaturation of the two meaſures " of the air under examination be completed, " the remaining column of air be found equi- " valent to three meaſures, and eight ſubdi- " viſions, or to 308 ſubdiviſions, this number " ſubtracted from the 500 parts, or ſubdivi-

" ſions of both airs employed, will give a re-
" ſult of 192, which is exactly the quantity
" of both airs deſtroyed. Let us now again
" ſuppoſe that the nitrous air employed was
" ſo weak that, inſtead of three meaſures, ſix
" were required before the ſaturation was
" fully completed, and that thus the remain-
" ing column of air in the great tube occu-
" pies 608, inſtead of 308 ſubdiviſions; we
" ſhall find that the reſult will be juſt the
" ſame, that is to ſay, that by ſubtracting
" the 608 parts remaining from the 800
" parts of both airs employed in the experi-
" ment, there will be found exactly 192
" ſubdiviſions deſtroyed, and that thus, in
" both caſes, the accurate ſalubrity of the
" air is aſcertained. If ſuch bad nitrous air
" only was at hand as juſt now ſuppoſed, it
" follows that a longer tube ought to have
" been employed. This obſervation," he adds,
" which I owe entirely to the Abbe Fontana,
" is in my opinion of the utmoſt conſequence,
" and throws a great deal of light upon the
" nature of nitrous air, and upon its won-
" derful property of deſtroying reſpirable air;

"and it illuſtrates his ingenious theory of "this quality, which I hope the author will "ſoon publiſh; but which I have no right "either to claim, or to anticipate. In con"ſequence of this obſervation, we need not "be ſo anxious about the goodneſs of the "nitrous acid, nor about the ſtrength of the "nitrous air."

In anſwer to this, I would obſerve, that if three meaſures of the nitrous air, in conſequence of which 500 ſubdiviſions of the graduated tube were reduced to 308, were not ſufficient to produce the greateſt diminution, three equal quantities more, having, by the ſuppoſition, a greater effect upon the common air, muſt neceſſarily make a greater difference than between 608 and 800. For if the difference ſhould be juſt 608, it would appear that the addition of more nitrous air had not been capable of making any change in it at all, contrary to ſuppoſition. Conſequently, if the air to be examined had not been completely diminiſhed before, ſo that the addition of theſe three meaſures would have produced a farther diminution, the quantity remaining muſt have been leſs than 608 ſubdiviſions of the tube.

This ingenious obſervation appearing to be ill founded, as well as contradicted by fact, it behoves us to be as attentive to the ſtrength of the nitrous air that we make uſe of, as a teſt of the purity of the other air, as ever.

It is true that we are all naturally biaſſed in favour of our own peculiar methods; but, beſides that of the Abbe Fontana is exceedingly operoſe and tedious, I do not ſee that it has, in any reſpect, the advantage of the very ready and ſimple method that I have hitherto made uſe of, and deſcribed in the introduction to my laſt volume. His making the inſide of his meaſuring tube rough with emery, would be a great advantage, if, as he ſays, it prevented the water from adhering to it in drops, and thereby contracting the dimenſions of the tube. But Dr. Falconer informs me, that he has prepared a tube in this manner, without finding any ſuch deſirable effect from it.

## SECTION XX.

### *Of the production of nitrous air in which a candle will burn.*

IN each of the four preceding volumes on the ſubject of air, I have treated of a ſpecies of nitrous air, procured ſometimes by a direct proceſs, but originally by a change in the conſtitution of common nitrous air, in which a candle burns either quite naturally, or with an enlarged flame, and ſometimes alſo with a crackling noiſe, and vehemence, as if it was true dephlogiſticated air, or a mixture of dephlogiſticated and inflammable air. In every ſucceſſive volume will be found ſeveral advances in the inveſtigation of the nature and properties of this ſpecies of air, in conſequence of my having given to it perhaps more attention than it will be found to deſerve. But our attention is by no means always beſtowed according to the real, or even the ſeeming importance of the objects of it, but to ſomething in them that excites our curioſity. Now there is ſomething ſo exceedingly remarkable in this ſpecies of air, eſpecially its property of admitting a candle to burn in it, when it is ſtill as fatal to animal life as any ſpecies of air whatever, that I have not been able to refrain from attending to it. This attention I ſhall ſtill keep up, and though I am far from having ſatisfied myſelf with reſpect to it, my readers will find in this volume many new obſervations relating to it, and particularly a moſt eaſy method of producing it in the greateſt quantity; whereas it was originally a tedious and uncertain proceſs with me.

My conjectures concerning the conſtitution of this kind of air will be found to have been various. But having diſcovered it at one time in a continued proceſs, of the ſolution of zinc in ſpirit of nitre, always to come between the phlogiſticated and dephlogiſticated air, I ſuppoſed it to contain leſs phlogiſton than phlogiſticated air, and more than the dephlogiſticated air; and therefore as it has ſome properties of a genuine dephlogiſticated air, I am inclined, on the whole, to call it *a dephlogiſticated nitrous air*. But if I adopt a language that ſeems to be authorized by my late experiments, I ſhould rather ſay that this kind of air conſiſts of a *dephlogiſticated nitrous vapour*, diffuſed through a quantity either of nitrous or phlogiſticated air; which nitrous vapour may be ſeparated from the nitrous, or phlogiſticated air, by water, it being abſorbed by it, in the ſame manner as fixed air.

It will not appear ſurpriſing, however, that having originally produced this air in my attempts to throw more phlogiſton into nitrous air than it naturally contained, and in the very ſame proceſſes by which common air actually does receive phlogiſton, as by means of iron filings and brimſtone, liver of ſulphur, &c. and it being alſo fatal to animal life, I was at firſt inclined to call it a *phlogiſticated nitrous air*, or, on account of its remarkable property of admitting a candle to burn in it with an enlarged flame, an *inflammable nitrous air*.

Since a candle burns in this kind of air, and an animal dies in it, I think we are authorized to ſay, that it is of ſuch a conſtitution, as to be capable of receiving phlogiſton in a very great degree of heat, perhaps not ſhort of a red heat, but not in that degree which is compatible with animal life. It is well known that many ſubſtances in chemiſtry can act upon one another, when they are hot, which do not at all affect one another when they are cold. This, therefore, may be the caſe with this kind of air, and ſubſtances containing phlogiſton.

Again, it is evident, from this air being readily diminiſhed by agitation in water, almoſt, if not quite as much as fixed air is, and then becoming phlogiſticated air, that that part of it which is capable of combining with phlogiſton, has a conſiderable affinity to water, and is thereby capable of being entirely ſeparated from the reſt of the air in which it is found. It is not, however, fixed air, both becauſe it does not extinguiſh a candle, and becauſe it doth not make lime water turbid. I ſhould think myſelf happy, if I could procure a quantity of this air, or vapour, quite pure, unmixed with nitrous or phlogiſticated air; but though I have made ſeveral attempts for this purpoſe, they have hitherto been without ſucceſs, except by firſt ſaturating water with the air, and then expelling it by heat.

I ſhall begin the recital of my late experiments on this difficult ſubject, with mentioning ſome unſucceſsful attempts to procure it, and then the method I at length hit upon for procuring it in the greateſt abundance.

Having firſt found this air by expoſing nitrous air to a large ſurface of iron, then to liver of ſulphur, to iron filings and brimſtone, and by other proceſſes which may be denominated *phlogiſtic*, becauſe a quantity of phlogiſton is ſuppoſed by chemiſts to be exhaled in them, I expoſed nitrous air likewiſe to a large ſurface of *white paint*, by laying it on thin pieces of wood, and then introducing them into a jar of that air.

In about two months one third of the nitrous air, in theſe circumſtances, diſappeared, but I only found that its power of diminiſhing common air was much impaired. For with an equal quantity of common air, the meaſures of the teſt were 1.48; but a candle would not burn in it. After one month more the meaſures of the teſt were 1.55; but ſtill it extinguiſhed a candle; and ſeeing no farther diminution of this air, I put an end to the proceſs. It is poſſible, however, that had the ſtate of this air been accurately traced, that ſtate of it in which a candle would burn in it might have been found. But the beſt chance of finding this air, in what may be called a *phlogiſtic proceſs*, muſt be when the diminution is pretty quick. For, otherwiſe, as faſt as this vapour which combines with phlogiſton is formed, it is abſorbed by the water.

For ſome time I thought I ſhould have been able to make this peculiar ſpecies of nitrous air by mixing nitrous air ready formed, with ſome other kind of air; and conſidering that iron expoſed to nitrous air becomes a calx, and therefore had parted with its phlogiſton, and poſſibly in the form of inflammable air, I mixed inflammable air with nitrous air in various proportions, but without effect. This mixture, I obſerved, burned in the neck of the phial, that is, by the help of common air, with a green or yellow flame. But the appearance is very unlike that which is produced by this air, which admits a candle to burn in it, without any aſſiſtance from common air.

I then thought it poſſible that what I could not produce by an immediate mixture might be effected by the ſame mixture in length of time. Accordingly I mixed nitrous and inflammable air in various proportions, and kept the mixtures many months; but this alſo was without effect. In ſome of the caſes I made uſe of inflammable air from the marſhes, which burns with a lambent flame, but with this I ſucceeded no better.

All theſe mixtures were diminiſhed as nitrous air alone would have been. It was, therefore, the ſame thing in effect as throwing a quantity of phlogiſticated air into the mixture, which in ſome meaſure varied the appearance of the accenſion. But except this, which I do not think worth noticing, nothing remarkable occurred in conſequence of keeping theſe mixtures ſo long. It is plain, indeed, that inſtead of getting an addition of phlogiſticated air, or of phlogiſton in any form, I wanted a ſubſtance with which phlogiſton might combine, and therefore a thing of a quite oppoſite nature.

At length I ſucceeded in a method of producing this air, when I expected quite a different reſult. As iron will precipitate copper from a ſolution of it in ſpirit of nitre, and I had obſerved air to be generated in this proceſs; I imagined that by putting iron into that ſolution of copper in ſpirit of nitre, I ſhould get more nitrous air, and with leſs expence. But inſtead of this, I procured what I ſhould much more have wiſhed for, viz. this new ſpecies of nitrous air. But before I hit upon this, I ſucceeded in the method of changing freſh nitrous air into this ſpecies of it, in a remarkably ſhort ſpace of time.

Having at hand a phial filled with nails, which had often been employed in diminiſhing nitrous air, I filled it up with a diluted ſolution of copper in ſpirit of nitre, and left it all night. I then diſplaced the liquor by nitrous air, and in about two hours the whole quantity was diminiſhed one half, and a candle burned in the remainder with an enlarged flame.

In this experiment I collected no air from the iron itſelf; but now, with the view that I have mentioned above, I filled the phial containing the nails with the ſolution of copper in ſpirit of nitre, and inverting it the next morning I found the phial full of air, and it was not proper nitrous air, as I had expected, but that ſpecies of it in which a candle burns. In this caſe it burned quite naturally, and without any enlarged flame.

The quantity of this peculiar ſpecies of nitrous air that may be procured in this manner, and from the ſame materials, viz. without changing either the iron or the ſolution of copper, is aſtoniſhing. I made however, a pretty full trial of it, in a jar which I had thruſt full of long pieces of iron wire, on purpoſe to expoſe as much of the ſurface of the iron as I poſſibly could, to any kind of air that I ſhould afterwards put into it. This jar, which held about a pint and a half, I filled with the diluted ſolution of copper, and inverted it in a baſon of the ſame ſolution; when, at the firſt, the jar was quite filled with this air in a few hours.

However, having got as much of this air as I wanted, I obſerved that more air was ſtill generated; and inverting the jar every day, after I had filled it with the ſame ſolution of copper that had been expelled by the generated air the preceding day, it never failed to produce a jar full of that air every day for at leaſt a fortnight, beſides what eſcaped towards the beginning of the proceſs from the mouth of the jar, as ſoon as it was full, and which I never collected.

I had imagined that, in time, the quality of this air would change, but to the very laſt it yielded air of the ſame kind in which a candle burned, not only naturally, but in a very vivid manner. If, however, I ſuffered the air to continue long in the jar, I always found it to be phlogiſticated air; which, indeed, is the ſtate to which this ſpecies of air is always reduced by long expoſure in the ſame circumſtances in which it is generated; the water either abſorbing the dephlogiſticated vapour, as it may be called, or this vapour getting ſaturated with phlogiſton.

It was ſomething remarkable, that a phial of nails which had often been uſed to diminiſh nitrous air, when filled with water only, yielded phlogiſticated air. I at firſt filled this phial with inflammable air, and had obſerved a conſtant addition to it. It required, however, the warmth of a fire, when the phial was filled with water, to make it produce any conſiderable quantity of phlogiſticated air. Theſe nails had probably a quantity of nitrous acid mixed with their ruſt, which might continue to act upon them; and this air, in its naſcent ſtate, might be nitrous, but afterwards became phlogiſticated, according to the uſual courſe of this proceſs.

## SECTION XXI.

### *Of the conſtitution of dephlogiſticated nitrous air.*

ONE of the moſt uncertain circumſtances attending this ſpecies of air which is the ſubject of the laſt and preſent ſection, is that ſometimes it will diminiſh common air, almoſt as much as freſh made nitrous air, and ſometimes it has no ſuch power; and I have not yet been able to ſay before hand when it will be poſſeſſed of this power and when it will not. As this dephlogiſticated vapour of nitre is a thing quite diſtinct from the air with which it may be mixed, this may be either proper nitrous air, or phlogiſticated air, that is, in this caſe, phlogiſticated air produced from nitrous air. For the progreſs of the production of this air ſeems to be as follows. Part of the nitrous acid is firſt looſed from that union with phlogiſton which conſtitutes nitrous air, and then becomes what I call *dephlogiſticated nitrous vapour*, diffuſed through the remaining nitrous air. In time the whole of the nitrous air is thus decompoſed, ſo that no proper nitrous air remains in the mixture. In this ſtate I imagine the air ſhews the ſtrongeſt appearances of dephlogiſticated air, by admitting a candle to burn in it with a vivid flame. But when the nitrous air has ſuffered that decompoſition, which lets the dephlogiſticated vapour of nitre eſcape, this vapour is either imbibed by water, or forms with the phlogiſton another kind of union, viz. that which conſtitutes phlogiſticated air; and when the whole of the nitrous air has undergone this change, having parted with all the phlogiſton which it held *as nitrous air*, it is no longer capable of attracting common air. Laſtly as the dephlogiſticated vapour is either abſorbed or ſaturated, the proportion of phlogiſticated air is greater, till at length it conſiſts of nothing elſe.

Whether this theory be ſupported by the following facts, in which this air ſometimes did, and ſometimes did not affect common air, I leave to the judgment of my readers.

Diſſolving zinc in ſtrong ſpirit of nitre, I got a conſiderable quantity of air in which a candle burned very vigorouſly, hardly to be diſtinguiſhed from dephlogiſticated air, and it had no effect on common air. But at other times I have got air from this ſolution that was pretty ſtrongly nitrous.

Nitrous air expoſed to iron from the 24th of June, and diminiſhed to about one third, admitted a candle to burn in it with a flame ſomewhat larger than natural, very bright, and ſlightly blue all round. In this ſtate, I doubt not, it would have diminiſhed common air, but when it was examined again on the 17th of July, it was neither diminiſhed by nitrous air, nor did it affect common air in the leaſt.

Two quantities of nitrous air expoſed to iron the 23d of July were diminiſhed between one half and one third on the 31ſt of the ſame month, when they admitted a candle to burn in them with a very vivid flame, and did not diminiſh common air at all.

Laſtly, a quantity of nitrous air, in contact with iron from the 2d of September was diminiſhed one half; and on the 13th of the ſame month it affected common air very little, the meaſures of the teſt being 1.75. It burned with an enlarged flame.

In all theſe caſes, in which the common air was not much, if at all affected by this air, a candle burned in it very vigorouſly; all the nitrous air being decompoſed, and not much of the dephlogiſticated vapour abſorbed. In the remaining trials the common air was affected.

A quantity of this air produced by a ſolution of tin, and in which a candle burned with a blue flame ſurrounding it, diminiſhed common air in part. This blue flame is an indication of a mixture of ſome proper nitrous air. For with that colour a candle is always extinguiſhed in nitrous air.

A quantity of this air (I did not note how it was procured) in which a candle burned with a bright flame, ſurrounded with a thin blue one, diminiſhed common air almoſt as much as freſh nitrous air. By ſtanding all night in water, about one fourth of it was abſorbed, but ſtill a candle burned in it naturally, and it diminiſhed common air not much leſs than before. In leſs than twelve hours afterwards, it extinguiſhed a candle, and then did not affect common air near ſo much.

Theſe facts are not ſo agreeable to the preceding theory as one might wiſh; yet this blue flame was an indication of nitrous air, and the quantity of air abſorbed ſhews that there was much dephlogiſticated vapour mixed with it, which ſupported the bright flame in the centre. The diminution in this caſe might be the effect of the abſorption of the dephlogiſticated vapour, of which this air contained ſo much. An attention to the redneſs of the mixture of it with common air might have helped to diſcriminate in this caſe. The next obſervation is ſimilar to the above.

A quantity of this kind of air in which a candle burned with a vigorous white flame, ſurrounded by a thin blue one, diminiſhed common air ſo much, that the meaſures of the teſt were 1.26. The next day this air, having ſtood expoſed to water all night, the manner in which a candle burned in it was not ſenſibly different, though one third of the whole quantity had been abſorbed. The meaſures of the teſt then were 1.24. The next day one half of what remained was abſorbed, and a candle went out in the air. The meaſures of the teſt were 1.34. The day following the air was diminiſhed one fifth more, and the meaſures of the teſt were nearly the ſame. This muſt have been pure nitrous air, mixed with very much dephlogiſticated vapour.

Another quantity of this air, in which a candle burned with a ſtrong white flame, in the centre of a light blue one, diminiſhed common air ſo much, that the meaſures of the teſt were 1.28. The next day it was diminiſhed about one ſixth, and juſt extinguiſhed a candle. Mixed with common air the meaſures of the teſt were 1.3. It appears from this experiment, that a very ſmall quantity of this dephlogiſticated vapour ſuffices for the burning of a candle in the air with which it is mixed.

A quantity of this air that approached the neareſt to the ſtate of dephlogiſticated air, with reſpect to a candle burning in it, was ſome that had been nitrous air, had been expoſed to iron the firſt of July, and was examined on the 17th. In this caſe the flame was exceedingly bright, attended with a crackling noiſe, inſomuch that, had I not known the manner of producing this air, I ſhould have pronounced it to be proper dephlogiſticated air, and yet it would certainly have been fatal to animal life.

Having tried the effect of the flame of a candle in this air, I had little doubt but that inflammable air might be ignited in it; nor was I diſappointed. For when the two kinds of air were mixed, they exploded at once, and with conſiderable violence, almoſt equal to that of a ſimilar mixture of inflammable and dephlogiſticated air.

I then put a pot of iron filings and brimſtone into a jar of this air, to try what alteration would be made in it by ſuch a gradual and equable ſupply of inflammable air, as that mixture would throw into it. After continuing in this ſtate four or five days, I found the air, though confined by water, very little diminiſhed. However, a candle then went out in it. It was farther diminiſhed by agitation in water. But I always found this air capable of being conſiderably diminiſhed by agitation in water, after it had been ſo far affected, by a previous ſtanding, or agitation, in water, that a candle would juſt go out in it.

That it is a mixture of pure nitrous air that is the cauſe of this thin blue flame with which the central flame in theſe experiments is ſometimes ſurrounded, is, I think, evident from the following experiment. Having a quantity of this air in which a candle burned with a ſtrong and bright flame, I mixed nitrous air with it, and then the candle burnt in it with the uſual enlarged flame, a bluiſh flame ſurrounding the former.

In order to determine as nearly as I could, how the property of admitting a candle to burn in this air, or of extinguiſhing it, correſponded to the quantity of its diminution, and alſo of that of the common air by means of it, I made the following experiments, which however, when compared with thoſe which have been mentioned before in other views, only give a general idea of this progreſs; and great varieties, I doubt not, may be found in different quantities of this air, in all the reſpects abovementioned.

Having prepared a quantity of this air by keeping nitrous air in contact with iron twelve days, I found that it bore agitation in water till near one half of it was abſorbed, before it would extinguiſh a candle.

Having again put a quantity of nitrous air to iron the 21ſt of September, the next day its power of diminiſhing common air was not ſenſibly changed. How much it was diminiſhed I did not note, but it was probably one tenth of the whole. On the 23d the diminution was one twelfth of the remainder, and with an equal quantity of common air, the meaſures of the teſt were 1.25; when, with freſh nitrous air, they

were

were 1.3. In this caſe part of the diminution was probably an abſorption of the dephlogiſticated vapour. On the 25th the diminution was one ſixth of the remainder, and the meaſures of the teſt were 1.37; by which it appears that this air had now loſt ſome of its power of diminiſhing common air. On the 26th the diminution was again one ſixth of the remainder, and the meaſures of the teſt were 1.42. On the 28th the diminution was one eighth of the remainder, and the meaſures 1.7. Dipping a flame of a candle into a portion of it, it burned with an enlarged flame. On 30th the diminution was again one eighth of the remainder, and the meaſures 1.8. Laſtly, on the 2d of October, the diminution was one ſixth of the remainder, and the meaſures about the ſame as before; ſo that now the diminution of common air by it was little or nothing. What now remained of the air was, I ſuppoſe, too ſmall to dip a candle into it, but I take it for granted that it would have been extinguiſhed.

Among other methods in which I attempted to procure this dephlogiſticated vapour quite pure, and freed from that part of the air with which it was mixed, and which either diminiſhed common air, or extinguiſhed a candle, that which I thought the moſt likely to ſucceed, was to ſaturate a quantity of water with this air, and then to expel it by means of heat. For then nothing could come out of the water but that part of the air which had been imbibed by it, of which the nitrous air, and eſpecially the phlogiſticated air, would be a very ſmall proportion.

Accordingly I took diſtilled water, and made a quantity of it abſorb as much as it would of this air; but when I expelled it again by heat, it admitted a candle to burn in it only juſt as it had done before. It was not converted into any better kind of air by the proceſs; for it was not at all diminiſhed by freſh nitrous air. I had entertained ſome ſmall expectation that if I failed to get pure dephlogiſticated vapour from this water, the air might come out changed in ſome other reſpect.

This experiment was made on the 4th of October. On the 13th of the ſame month I expelled air from what remained of that water, after it had been kept in a phial with a ground ſtopper, half filled with common air; when the air expelled from it was found to be much leſs in quantity than before, and it was diminiſhed by nitrous air about as much as common air. By this it appears that the quality of it was conſiderably altered by its *continuance* in water; and alſo, probably, by means of the air incumbent upon the water; but in what manner this was effected, it will require more experiments to explain.

On the 13th of October I repeated this experiment, by agitating a quantity of this nitrous air in a ſtate in which it did not affect common air at all, in diſtilled rain water; and expelling the air *immediately afterwards*, a candle burnt in it with a vivid flame, as before; and after the water had abſorbed what it would of the air, a candle went out in the remainder, ſo that it was, in all reſpects, the ſame thing when it came out of the water, that it had been before it went into it. Perhaps, however, a leſs degree of heat might have expelled one part of this air, without the other. In proper circumſtances that is, when confined ſo as not to be expoſed, through the medium of water, to the common atmoſphere, this kind of air remains unchanged both in quantity and quality, as much as any other ſpecies of air whatever. Having kept different quantities of it in a great variety of circumſtances, from different periods, to the 24th of July, when I was obliged to put an end to the proceſs, I took the following notes reſpecting this ſubject.

Two quantities of this air, which had been kept many months in phials with ground ſtoppers, admitted a candle to burn in them with a bright enlarged flame. One of theſe portions of air, agitated in water, was preſently reduced to very near one half its bulk; and then, with an equal quantity of common air, the meaſures of the teſt were 1.66. The other extinguiſhed a candle when, without agitation, it was diminiſhed about one tenth only.

Another quantity of this air, expoſed to an equal bulk of water, upon quickſilver, and frequently agitated, but which had not been ſenſibly abſorbed by the water, admitted a candle to burn in it quite naturally. Alſo another quantity, which had been procured from iron by means of the diluted ſolution of copper in ſpirit of nitre, and in which a candle burned naturally the third of October 1779, and which had been confined by quickſilver, appeared to be, in all reſpects, unchanged at this time.

In vol. ii, p. 130, I have given an account of a mixed kind of air, from ſpirit of nitre and oil of turpentine, to which, when a quantity of alkaline air was introduced, a white cloud was made, and part of the air diſappeared. In this air a candle burned both before and after that proceſs. This air, no doubt, contained a mixture of dephlogiſticated nitrous air; but the white cloud muſt have been owing to a mixture of fixed air in it. For repeating the experiment with dephlogiſticated nitrous air, procured from iron and a ſolution of copper in the nitrous acid, which contained no fixed air, I did not perceive the leaſt cloud when alkaline air was admitted to it, nor the ſmalleſt diminution in the quantity of the whole, while they were confined by quick-ſilver. When water was admitted to them, it abſorbed the alkaline air, and a candle burned in the remainder exactly as before.

This proves that the acid in dephlogiſticated nitrous air is intimately combined with ſome other ſubſtance. The ſame appears, perhaps, ſtill more clearly, by means of the juice of turnſole. For admitting a quantity of this air to water tinged blue with the juice of turnſole, part of it was abſorbed, in one caſe about one half of the whole, without making any change in the blue colour.

## SECTION XXII.

### *Of the production of inflammable from alkaline air; by the electric ſpark.*

THERE are few experiments the *rationale* of which I leſs pretend to underſtand, than the production of genuine and permanent inflammable air from alkaline air, by means of the electric ſpark; the alkaline air being wholly imbibed by water, and the inflammable air produced in it, if not from it, not at all.

One query on this ſubject is, whence comes the phlogiſton, which is certainly a principal ingredient in the conſtitution of inflammable air. Alkaline air, indeed, contains phlogiſton, becauſe, in the manner in which I have generally produced it, it is itſelf partially inflammable; but it is not nearly ſo much ſo as the inflammable air, which is produced by means of it. Beſides, it will appear by the following experiments, that the quantity of the inflammable air far exceeds that of the alkaline. If I might indulge a conjecture on the ſubject, it ſhould be, that the phlogiſton of this inflammable air is ſupplied by the electric matter, and that ſomething which ſerves for a *baſis*, as it may be called, is ſupplied by the alkaline air. For though inflammable air approaches the neareſt to the ſtate of pure phlogiſton of any ſubſtance that is known to us, it is, I doubt not, compoſed of phlogiſton and *ſomething elſe*; and this experiment may perhaps ſhew, that this baſis, which has hitherto been unknown to us, is of an alkaline nature. For taking the electric ſpark in any ſpecies of acid air, has no ſuch effect. From this fact alſo we may perhaps be led to imagine, that phlogiſton has a nearer relation to an acid nature than to an alkaline one; ſo that it readily combines with the latter, as with the earth of metals, &c. rather than the former.

At the time of my former publication on this ſubject, I had only aſcertained, though very deciſively, the general fact, viz. the undoubted production of a true inflammable air, exactly like that which is produced from iron or zinc by ſolution in oil of vitriol or ſpirit of ſalt, in conſequence of taking the electric ſpark in alkaline air. But I have ſince aſcertained the *quantity* of inflammable air that may be produced from any given quantity of alkaline air. And this production having its limits, certainly ſhews that the alkaline air ſupplies ſome eſſential part in the conſtitution of it, and that the whole of the inflammable air is not derived from the electric matter, the alkaline air ſerving only as a medium, as it might otherwiſe have been imagined, in which the proceſs takes place.

To take my meaſures with more accuracy, I, at this time, confined the alkaline air in a glaſs tube, of the ſame dimenſions throughout; and having it confined, as it neceſſarily muſt be, by quick-ſilver, I carefully marked the ſpace which it occupied in the tube. I then took the electric ſpark, or exploſion, which ever of them happened to be the moſt convenient, till I perceived that no more addition was made to the quantity of air; and then meaſuring the ſpace which it occupied, I found that the whole was, as nearly as poſſible, three times as much as that which the alkaline air alone had occupied. When I examined this air, I found it to have an inflammability of the ſtrongeſt kind, firing with exploſions, and in no reſpect to be diſtinguiſhed from that which is extracted from metals by acids. Alſo the electric ſpark taken in this air was always red, though, as is alſo the caſe with other inflammable air, it was white in the centre of any conſiderable exploſion taken in it.

If the theory ſuggeſted above be the true one, this experiment may prove that the proper inflammable principle, ſupplied by the electric matter, conſtitutes two thirds of the bulk of inflammable air, and the alkaline principle only the other third. We are not authorized however, to infer from it any thing at all concerning the ſeparate *weights* of the alkaline and inflammable principle. It is even poſſible that the phlogiſton may have no weight at all, though it enables the alkaline baſe, on which it has ſeized, to occupy ſo much more ſpace than it did before.

After this experiment it ſtill remained a doubt whether, when the proceſs was completed, there did not remain, at leaſt, ſome portion of alkaline air not affected by it, and capable of being abſorbed by water afterwards. To determine this, and likewiſe to repeat ſo important an experiment upon a larger ſcale, I began with one third of an ounce meaſure of alkaline air, and I took the electric ſpark in it till I had got a complete ounce meaſure of air. Then admitting a little water to it, I obſerved with the greateſt attention, but could not perceive that any part of the air was abſorbed by it. However, when I had made this air explode, by means of the flame of a candle, and immediately after applied my noſtrils to the mouth of the veſſel in which it had been contained, I perceived a very evident alkaline ſmell; ſo that the whole of the volatile alkali had not been completely incorporated with this air, though it was ſo much ſo, as not to be ſeized by the *water*. And to give it a fairer trial, this water had been confined along with the air, upon the quick-ſilver, and had been even frequently agitated with it, during two whole days; and though it was but a very ſmall quantity, it had no perceivable alkaline ſmell afterwards.

If the theory concerning the conſtitution of inflammable air, advanced by way of conjecture above, be admitted, it will favour the ſuppoſition in my firſt volume, p. 106, that part of the calx of the metal, from which the inflammable air was produced, enters into the conſtitution of the air, and is the proper baſis of it; the calces having ſome properties of an alkaline ſubſtance. It may alſo prove the convertibility of one alkaline ſubſtance into another, or at leaſt ſuch a change as makes them to become the ſame thing, that is, to have all the ſame known properties in the conſtitution of air; in the ſame manner as there is ſomething common to the vitriolic and nitrous acids, which cannot be diſtinguiſhed when they enter into the conſtitution of dephlogiſticated air. Since inflammable air, when procured from any of the metals, by any of the acids, or from alkaline air by the electric ſpark, exhibits, in all reſpects, the very ſame properties, it can hardly be imagined that its compoſition is really, or, at leaſt, materially, different. But this cannot be aſcertained completely till we we are able to decompoſe any kind of air, and collect again the different elements out of which we had previouſly formed it; and this is a problem, of the ſolution of which, after what has been already done on this ſubject, we ought not to deſpair.

Do not theſe experiments lead us to conjecture, that the inflammability of alkaline air, does not ariſe from the *alkaline principle* itſelf, or from any thing neceſſarily connected with it, but from phlogiſton accidently adhering to the materials from which it was expelled, readily forming with this air, as a baſis, a ſpecies of inflammable air; and will not phlogiſton ſet looſe in other proceſſes have the ſame effect in alkaline air?

## SECTION XXIII.

### *Experiments proving the great volatility of quick-ſilver.*

THAT Mercury is volatile, even in the temperature of the atmoſphere, when its ſurface is expoſed to a *vacuum*, has been long evident from obſervations on the barometer, in ſome of which, expoſed in the ſun, a perfect diſtillation is perpetually going on; the inviſible mercurial vapour always riſing on the warmer ſide of the tube, and then forming into globules, and running down the oppoſite ſide, in the form of denſe fluid mercury. But the experiments I have lately made ſeem to ſhew that this heavy ſubſtance is not leſs volatile when confined by vitriolic acid air, though preſſed with the weight of the atmoſphere, and that it is in ſome meaſure volatile, even when expoſed to common air.

Preſently after the diſcovery of vitriolic acid air, I obſerved that when the electric exploſion

ſion was taken in it, which was done by confining it with quick-ſilver, in a glaſs ſyphon, ſo that the electric matter was made to paſs from the mercury in one leg of the ſyphon to the mercury in the other, the tube was preſently covered with a black incruſtation, and the longer the exploſions were continued, the thicker this incruſtation grew. I had not at that time, however, any ſuſpicion that this black matter came from the quick-ſilver, but imagined that it was altogether formed from the vitriolic acid air. This I was then led to conclude from there being no ſuch appearance when the electric ſpark was taken in marine acid air, though confined by mercury, in the very ſame manner.

Afterwards, obſerving the ſame black matter, though not procured with the ſame eaſe, or in ſo great a quantity, when the exploſion was taken over mercury in common air, I could not help ſuſpecting that this black matter came from the mercury; and this ſuſpicion was confirmed by applying heat to it; for it was thereby converted into white fluid mercury. I thought, however, that it was produced by the electric exploſion volatilizing the mercury, in conſequence of falling directly upon it. For though the heat occaſioned by ſuch an exploſion be confined to a ſmall ſpace, it is exceedingly intenſe.

That the exploſion might not affect the fluid mercury, I next took it between two iron wires, half an inch above the ſurface of the mercury, in the vitriolic acid air confined by it, and ſtill had the black matter; which made it evident that the electric exploſion did not produce the evaporation of the mercury, but found the mercurial vapour diſperſed in the air. I alſo made the ſame experiment, and with a ſimilar reſult, in common air. But in this caſe I could not produce the black matter, at leaſt in any ſenſible quantity, at any conſiderable diſtance above the ſurface of the mercury; and in no reſpect were the appearances ſo ſtriking, as when the exploſions were taken in vitriolic acid air.

I took the electric exploſion between iron wires at the diſtance of ſeveral inches above the ſurface of the mercury in this kind of air, and the blackneſs within the tube was produced juſt as much as it had been when the exploſion was taken immediately upon the quick-ſilver itſelf; and on applying heat to the black matter formed in theſe circumſtances, it preſently became running mercury as before.

Having taken the electric exploſion at various diſtances above the ſurface of the mercury by which the vitriolic acid air was confined, and always with the ſame ſucceſs; I at laſt took it at the greateſt diſtance that any glaſs tube I had by me would admit, which was about three feet above the ſurface of the mercury. But even in this caſe the black matter was, to all appearance, produced quite as readily, as when the exploſions had been taken ever ſo near to the ſurface of the mercury; ſo that the mercurial vapour had completely pervaded this whole ſpace of vitriolic acid air, and in a very ſhort time; for I took the exploſions preſently after I had prepared the tube for the experiment.

But to be quite ſure that this black matter did not proceed from the vitriolic acid air, I contrived to take the electric exploſion in it when it was not confined by mercury. To do this, I completely ſaturated a quantity of water with this kind of air, confined in a glaſs tube, in the top of which I had cemented a piece of iron wire, which came within a proper diſtance of the extremity of another piece of wire, which reached to the bottom of the tube. The impregnated water was confined by mercury in the tube, and in the baſon.

In theſe circumſtances, a ſmall degree of heat made this water give out its air; ſo that all the upper part of the tube was filled with it, reſting on the water only. Between theſe two wires I took large electric exploſions a conſiderable time, but no black matter was produced. It is evident, therefore, that this black matter conſiſts of mercury *ſuperphlogiſticated*; the phlogiſton coming from the electric matter when the exploſions are taken in common air, but chiefly from the vitriolic acid air which abounds with it, when they are taken in that air; and this accounts for the appearances being ſo much more remarkable in this kind of air than in common air.

But though, in my experiments on mercury, an account of which is given in my laſt volume, mercury ſuperphlogiſticated by agitation in water, and aſſuming the form of a black powder, becomes white running mercury the moment that it becomes dry, this was not the caſe with the black matter in this proceſs. However, when I moiſtened a little of it, and dried it again, I thought that part of its blackneſs diſappeared, though not very ſenſibly.

Ethiops mineral is a compoſition of mercury and brimſtone, and therefore reſembles the black matter produced by theſe electric exploſions in vitriolic acid air, and the vapour of mercury; the vitriolic acid air alone, as I have ſhewn, becoming ſulphur in certain circumſtances. I thought, therefore, that this black matter might be a real ethiops; but when I put a little of it upon hot iron, I did not perceive any blue flame to ariſe from it. If, therefore, this black matter be an ethiops mineral, the proportion of ſulphur in it muſt be exceedingly ſmall.

It ſtill remained to be determined, whether this diffuſion of mercurial vapour through the vitriolic acid air was occaſioned by a proper *evaporation*, that is, by the repulſion of its particles, whereby it is made to aſſume an elaſtic form, and in that ſtate to mix with the air; or whether there be a *chemical union* formed by the mercury and this kind of air, and it therefore becomes *incorporated* with it. The following experiment ſeems to decide in favour of a proper evaporation.

I put a ſmall globule of mercury into a narrow glaſs tube, communicating with the inſide of the phial in which the oil of vitriol and copper for the production of vitriolic acid air were contained. But though I heated theſe materials, and continued the production of vitriolic acid air in theſe circumſtances a long time, ſo that the globule of mercury was always kept expoſed to a torrent of this kind of air, newly generated, I ſaw no proſpect of its being at all diminiſhed by it. I therefore conclude that vitriolic acid air does not properly take up, ſo as to combine with, the mercury. However, it muſt be acknowledged to be difficult to account for the quantity of mercury contained in this black matter in whatever manner it becomes diffuſed through the air, conſidering that this globule of mercury was not ſenſibly diminiſhed. This, however, might poſſibly be owing to its being continually ſurrounded with a little moiſture, from which I could not keep it free; owing perhaps to the oil of vitriol not being ſufficiently concentrated, ſo that the watery part was thrown off by the heat.

## SECTION XXIV.

### *Of the preſence of the nitrous acid in the calces of metals.*

ALL the nitrous metallic ſalts have been diſtinguiſhed by their property of *deliqueſcence*; but in my experiments with a long continued ſand heat, of which an account is given in my laſt publication, I produced two of theſe ſaline ſubſtances, which did not deliqueſce at all. They were produced from diluted ſolutions of copper and of mercury in the nitrous acid. The cryſtallizations were formed during the action of heat, in glaſs veſſels hermetically ſealed; and they were diſſolved again in the ſame menſtruum, when it was cold. But when the veſſels were broken, and the ſaline ſubſtances were expoſed to the air, they attracted no humidity at all; and yet they were not mere calces, becauſe they were exceedingly cauſtic, and had a moſt diſagreeable taſte. I have ſince produced a ſaline ſubſtance of this kind from *iron* in a much leſs ſpace of time, and the examination of it may throw ſome light on the conſtitution of the others.

A diluted ſolution of iron in nitrous acid, being only expoſed one day to a pretty ſtrong ſand heat, in a glaſs tube hermetically ſealed, all the iron ſeemed to be precipitated, and the liquor was left nearly colourleſs. This liquor afterwards diſſolved iron as before, ſo that the action of heat in theſe circumſtances, viz. under a ſtrong preſſure, and when nothing can eſcape into the open air, ſeems to oblige the acid to quit it's hold of the metal, in a great meaſure. It is indeed the property of nitrous ſolutions of iron, that they will always make a depoſit, and then diſſolve more iron, I believe without limits; but then the colour of the acid always continues red.

By this proceſs, therefore, this remarkable property of the nitrous acid ſeems to be increaſed with reſpect to iron, and may perhaps be extended to the other metals. I have not indeed as yet extended this operation to the other metals, and therefore it muſt at preſent be conſidered as a mere conjecture. But as I am now ſituated in a country where long continued fires will be leſs expenſive, I hope that, if all be well, my readers may hear from me again on this ſubject, and others of the ſame kind.

The iron precipitate was by no means a mere calx; for it had a very acrid taſte.

With copper a conſiderable time ſeems to be abſolutely requiſite to produce theſe non-deliqueſcent cryſtals, as appears from the following experiment which was likewiſe attended with ſome other circumſtances, that I am not able to explain. A quantity of a weak but ſaturated ſolution of copper in ſpirit of nitre, which had been expoſed to a ſand heat about a week, and in which ſome cryſtals were formed, had many more cryſtals formed in it; ſo as to become like a thin paſte, preſently after it was poured out of the tube. But when the whole maſs was diſſolved by heat, in the open air, and then dried, it became perfectly deliqueſcent; unlike that which had cryſtalized before in a longer continued heat.

That excellent philoſopher, and moſt amiable man, Mr. Fabroni, who is as communicative as he is intelligent, informed me that the calx of tin would dephlogiſticate ſpirit of nitre, and leave it colourleſs. This I found to be true; but then I found that, together with its colour, the acid loſt almoſt all its ſtrength. And trying other metals, I preſently found that the earths of all of them have a remarkably ſtrong affinity with the nitrous acid, and firmly uniting with it and a little water with which it is combined, make together a perfectly dry ſubſtance, quite unlike what it was before; the water being no more apparent, than it is in dry ſlaked lime. But heat will diſcover the water in both the caſes.

Of this kind of calx, which I think we may properly term *nitrated* is the white minium, which I had before procured by ſaturating red lead with nitrous vapour; the phænomena of which, as I have found them to extend to other metals, I now underſtand better than I did before. I thought it ſomething extraordinary, that a red ſubſtance,

ſtance, like minium, ſhould, by the addition of a red and highly phlogiſticated vapour, become a white ſubſtance. But I find that all the metallic calces on which I have tried the experiment do alſo become white, when they are, in like manner, ſaturated with ſpirit of nitre; and that this may be effected by a much eaſier proceſs than I thought of before.

The production of the red vapour of ſpirit of nitre by means of biſmuth, and other metals of which it makes a rapid ſolution, will be a difficult and unpleaſant proceſs to moſt perſons; and thoſe who are moſt expert in experiments of this kind, will be obliged to make ſeveral trials before they ſucceed to their wiſh, in ſome of the experiments that I have reported. But I now make all theſe nitrated calces by means of the ſimple diſtillation of weak ſaturated ſolutions of any of the metals.

In this proceſs the greateſt part of the water is evaporated, and the acid, together with a ſmall portion of the water, firmly unites with the calx of the metal, and, together with all the phlogiſton that the metal contained, is depoſited in the form of a white powder, which is incapable of being re-diſſolved, either in the ſame menſtruum, or in water. This depoſit of white matter is made during the whole courſe of the diſtillation, in which nothing comes over but water; and the whole of the metallic calx becomes a white nitrated powder, as deſcribed above. This, at leaſt, is the caſe with copper; and though I did not make the experiment in the ſame manner with *tin*, the phænomena, in a ſimilar proceſs, were the very ſame. There will probably, however, be conſiderable differences when the proceſs is extended to other metals; and if I go through with theſe experiments, I ſhall not fail to report them.

In diſtilling a quantity of that ſolution of copper, which remains after making nitrous air (of which about one twentieth part is ſtrong ſpirit of nitre, and the reſt water) but fully ſaturated, there came over a tranſparent liquor, which had little or no taſte; and from the very beginning of the proceſs, I obſerved a conſtant depoſition of white matter,

ter, which kept increaſing, till the greateſt part of the fluid was expelled. This matter I collected and dried, when it remained a perfectly white powder, but was eaſily diſcovered to contain much concentrated nitrous acid. For when I expoſed it to heat in a glaſs tube, it emitted a copious red vapour, together with a good deal of liquid, and exhibited all the phænomena that I had before diſcovered in white nitrated minium, and in the calx of tin, on which I had diſtilled ſpirit of nitre. For, beginning with the idea that Mr. Fabroni had given me, I firſt put the ſpirit of nitre upon the calx of tin, and afterwards upon the tin itſelf; but I had the ſame produce of white nitrated powder at the laſt. That calx of tin which was yellow was made perfectly white by the diſtillation of ſpirit of nitre upon it.

The experiment of lead I made in a different manner, as follows. I diſſolved ſeven dwts. of lead, in ſpirit of nitre mixed with about an equal quantity of water, when ſome air was produced, but not much. The bulk of what remained was a white powdery ſubſtance,

ſtance, covered with a ſmall quantity of liquid, at firſt green, but afterwards tranſparent. Transferring the whole into a cup, and rincing the phial in which the ſolution had been made, I obſerved that the white ſubſtance, which was *nitre of lead*, was immediately diſſolved by the water. Placing the cup in which the whole was contained near the fire, it became almoſt all liquid, and tranſparent, the menſtruum being enabled by heat to hold in ſolution a much greater quantity of this nitre of lead.

When by this expoſure to heat, all the moiſture was evaporated, and it was made perfectly dry, it weighed eight dwts. ſo that there was an addition of one dwt. from the acid and the water that were now latent in this calx. In this manner, however, it was brought to the ſame ſtate with the nitrated calces of copper and tin abovementioned. For when heat was applied to this white ſubſtance, a red vapour was expelled from it, but ſeemingly combined with more water.

Having, in this, or ſome other ſimilar manner, procured white nitrated calces of *lead*, *zinc*, *copper*, and *tin*, I incloſed a little of each in ſeparate glaſs tubes; and then, with a blow pipe, applied to them the flame of a candle; when they all emitted red vapour, and as ſoon as the tubes were quite filled with it, I cloſed them all hermetically, before any air could be admitted.

Letting theſe tubes remain ſome days, I obſerved that the red vapour was reabſorbed by all the calces, but leſs ſlowly by the calx of lead than by thoſe of tin or copper, and moſt quickly by that of zinc. N. B. I found it exceedingly difficult to expel all the moiſture from the ſolution of zinc in ſpirit of nitre; but when this was effected, I had a true nitrated calx of this metal, as well as of the reſt.

This experiment diſcovered to me a miſtake I was under with reſpect to my laſt directions for filling of glaſs tubes with the red vapour of ſpirit of nitre. Inſtead of doing it directly, from the ſolution of biſmuth, which is a difficult and diſagreeable operation, I adviſed to procure, in the firſt place, a quantity of what I now call *nitrated calx of lead*;

and putting ſome of it into a glaſs tube, cloſed at one end, to heat it till the whole tube be filled with the red vapour, and then immediately to ſeal it hermetically. This direction will ſtill be right, provided that preſently afterwards that end of the tube which contains the nitrated calx of lead be taken off, by melting the tube juſt beyond it, which indeed I then alſo adviſed to do, though I had not then diſcovered the principal reaſon for it. For if the white calx from which the red vapour was expelled be ſuffered to remain long in the tube, it will reimbibe the whole of it. But then the vapour may be expelled again by heat, and will continue to fill the tube a conſiderable time.

When I firſt produced the nitrated calx of lead, it was by means of a rapid ſolution of pieces of biſmuth; and the vapour was conveyed immediately from the veſſel in which the ſolution was made, through a bent tube connected with it, into the other veſſel, in which I had placed the red lead. But this vapour, as I then obſerved, was by no means dry; and ſmall drops of a very blue ſpirit of nitre were frequently falling from the end of the tube out of which the vapour iſſued. This degree of moiſture I find greatly facilitates the abſorption of the vapour.

Willing to try the effect of a perfectly *dry* nitrous vapour, I made the ſolution with the apparatus deſcribed vol. iii. fig. 3, interpoſing one of the inverted phials between the two veſſels thar I made uſe of before; and at firſt I concluded that this dry vapour would not be imbibed by the minium at all. But I found, after ſome days, during which it had been confined in a phial with a ground ſtopper, together with ſome minium, that it was completely abſorbed, and the red lead became white as before. I propoſe to repeat with this *dry* vapour moſt of the experiments which I formerly made with the *moiſt* vapour.

## SECTION XXV.

### *Of the mixture of nitrous and vitriolic acid.*

I HAD before obſerved ſome pretty remarkable phænomena that attend the mixture of the nitrous and vitriolic acid, particularly a turbid appearance, and a white depoſit, though both the acids were perfectly tranſparent. Thinking that this might poſſibly ariſe from ſome extraneous earthy matter, in the oil of vitriol, I repeated the experiment with a quantity which had been firſt diſtilled, and then concentrated, and with a nitrous acid the pureſt and the paleſt that I could make. But this mixture was attended with the ſame phænomena as before; namely with heat, and a turbid white depoſit.

I collected a quantity of this white depoſit, and found that it was compleatly diſſolved in ſpirit of ſalt, and gave it a yellow colour; ſo that it ſeems to be the ſame thing that is depoſited when the vitriolic acid is diſtilled to dryneſs, and therefore to be ſomething contained in this acid, and probably eſſential to it. This earthy matter ſeems to deſerve a more particular conſideration.

One of the moſt extraordinary circumſtances that I have hitherto obſerved relating to this mixture, is the extreme volatility that it ſeems to give to the nitrous acid, ſo that, as far as I can yet perceive, the whole of it makes its eſcape from the mixture. This obſervation was at firſt quite caſual. For having left the mixture, conſiſting of equal quantities of the ſtrongeſt kinds of each of theſe acids, in a phial with a ground ſtopper, about four months, in which I had been abſent from home, I found, at my return, the ſtopper driven out, and nothing in the phial beſides the vitriolic acid, and, as far as I could judge, quite pure. For when I diſſolved iron in it, nothing but inflammable air was yielded, even from the beginning of the proceſs, and no mixture of nitrous air at all. Alſo the vitriolic acid was much weaker than it had been; ſo that it had been diluted afterwards by imbibing water from the atmoſphere.

I had the ſame reſult from another mixture of equal quantities of the two acids, which had ſtood in the phial without a ſtopper from the 6th of June to the 23d of July; and the quantity was diminiſhed only one-fourth of the whole.

I alſo expoſed, during the ſame time, to the open air, ſome of the cryſtals which I had obſerved to be formed by the impregnation of the vitriolic acid with the nitrous acid vapour. The conſequence was, that the cryſtals gradually diſſolved, and the quantity of liquid increaſed, till it exceeded twice the bulk of the cryſtals. When I diſſolved iron in this liquid, I got nothing but inflammable air. When the very firſt produce of it was mixed with common air, there was no ſenſible diminution of it, ſo that there ſeemed to be no nitrous air produced.

Another method of ſeparating the nitrous from the vitriolic acid, and in much leſs time than the above, was by expoſing the mixture to nitrous air. This I have obſerved phlogiſticates nitrous acid, and renders it extremely volatile; ſo that a very great proportion of it eſcapes. And when it is mixed with the vitriolic acid, and expoſed in the ſame manner, the whole of it ſeems to eſcape.

Having introduced a phial of this mixture into a jar of nitrous air, in the ſame manner as I had before treated the nitrous acid itſelf, I obſerved that it abſorbed the nitrous air as faſt as the pure nitrous acid alone had done. Immediately after the proceſs was commenced, it was covered with a denſe red vapour, and gradually aſſumed a light orange colour throughout, beginning at the top. When the whole of it had acquired this colour, I withdrew it, and expoſed it twenty-four hours to the open air; after which the top was become of a light blue, and the bottom of a yellowiſh colour. I then put it into another jar of nitrous air, and ſuffered it to remain there a fortnight, during which time I was abſent on a journey.

At my return I found the mixture quite colourleſs, though it had abſorbed little more of the nitrous air. I then diſſolved iron in it, and it yielded nothing but inflammable air, of the ſtrongeſt kind, without the leaſt mixture of nitrous air; the very firſt produce of it not in the leaſt affecting common air. The water in the jar in which this proceſs was made yielding air copiouſly, I collected a quantity of it, and found it to be ſtrong nitrous air. It had been produced by the impregnation of the water with nitrous vapour.

In order to diſcover in what *time* this effect might be produccd, I repeated the experiment, and found that after being expoſed four days to nitrous air, it became colourleſs, and the air produced by it from iron was all inflammable air.

If the vitriolic and the marine acids be mixed, much, if not all, of the marine acid is preſently expelled, in the form of marine acid air. I was willing to try what would be the effect of adding this acid to the mixture of the two others abovementioned; and I obſerved that, when I had poured a ſmall quantity of a perfectly colourleſs marine acid, very gently upon the other, preſently after they had been mixed, and while they were yet turbid, the marine acid remained tranſparent upon them both; but the place of contact preſently became of a beautiful yellow or orange colour, very ſmall bubbles of air riſing now and then from it.

The next morning the whole mixture was of a beautiful orange colour. When it was agitated, it frothed very much, and the air or vapour, eſcaped very rapidly, making, as it were, ſmall exploſions; but after every agitation the mixture ſeemed to be more viſcid, the air eſcaping with more difficulty. After the agitation, it remained of a paler colour than before. Probably the marine acid air had been, in ſome meaſure, thrown out; and the next day it was perfectly colourleſs, like water.

Bits of paper and bits of wood were not ſenſibly affected by the mixture of nitrous and vitriolic acids, and they did not give it any colour; but a fly gave another quantity of it a browniſh tinge, though not very ſoon. The next day that in which the vegetable matter had been immerſed, was of a light blue, and that into which the fly had been put was ſtill of an orange colour, and rather deeper than before. Three weeks after this, both theſe mixtures having been a long time quite colourleſs, I diſſolved iron in them, and they both yielded inflammable air only; ſo that, if this be any proof of the abſence of the nitrous acid, this acid had now entirely left the mixture.

It may be doubted, however, whether this be a ſufficiently accurate teſt of the abſence of the nitrous acid; though, when I formerly mixed theſe two acids, in an experiment of which an account is given vol. iii. p. 171, I found that iron diſſolved in it yielded firſt nitrous, and then inflammable air, the former, no doubt, from the nitrous acid, and the latter from the vitriolic. I alſo found, in the courſe of theſe very experiments, that when I had kept a quantity of this mixture in a phial, with a ground ſtopper, from the firſt of June to the 23d of July, the firſt part of the produce of air by it, from iron, diminiſhed common air very much, and there was the appearance of ſomething nitrous even in the laſt produce. For when it was exploded, there was a tinge of yellow or green in the flame. There was nothing red in the ſolution afterwards.

I ſhall endeavour to aſcertain by other teſts, whether there be any nitrous acid remaining in theſe mixtures.

## SECTION XXVI.

### *Of the marine acid, and marine acid air.*

SEVERAL years ago Mr. Woulfe informed me, that he thought that, by operating in my way, I ſhould be likely to find ſomething remarkable in the ſolution of *mangăneſe* in ſpirit of ſalt; but, in a very friendly manner, he, at the ſame time, cautioned me with reſpect of the vapours that would iſſue from it, as, from his own experience, he apprehended it was of a very dangerous nature. He was alſo ſo obliging as to furniſh me with a quantity of manganeſe for the purpoſe. I cannot ſay that it was the apprehenſion of danger, but rather having other things in view, that prevented my giving much attention to the ſubject at that time; and I ſhould probably have deferred it ſtill longer, had not Mr. Fabroni informed me of the dephlogiſticating power of manganeſe with reſpect of ſpirit of ſalt, diſcovered by Mr. Bergman.

This information ſuggeſted a wiſh to procure a quantity of a perfectly dephlogiſticated marine acid, in order to ſatisfy myſelf whether it would then yield any acid air, as it does in its common ſtate, that is, when phlogiſticated; ſuſpecting that it would not, as I have always imagined that a certain portion of phlogiſton is neceſſary to all ſubſtances, and eſpecially acids, aſſuming the form of air.

The experiments that I have made upon this ſubject give much weight to this opinion, and at the ſame time throw great light on the general doctrine of theſe kinds of air. For it appears that the marine acid, when it is deprived of its phlogiſton, is brought into a ſtate very nearly reſembling the nitrous acid; being then incapable of being exhibited in the form of air, that is, of air capable of being confined by quickſilver. For the moment that the vapour, which then iſſues from it, is admitted to quickſilver, it unites with it, and forms a white powdery ſubſtance, in the ſame manner as the nitrous acid vapour does; and when I reſume theſe experiments, I ſhall probably find that, with oily and other ſubſtances, this dephlogiſticated marine acid vapour will form compounds equally ſimilar to thoſe formed with them by the nitrous acid vapour. This is a new field that is yet before me.

From this analogy it is evident, that nothing is wanting to the nitrous acid vapour, to its aſſuming the form of *air*, but a ſufficient quantity of phlogiſton; and when it has got this phlogiſton, it is *nitrous air*. This, therefore, is probably the neareſt approach that we ſhall ever make towards bringing the nitrous acid into the form of air; and it is probably the combination of ſo much phlogiſton with this acid, in the compoſition of nitrous air, that makes it not ſo readily abſorbed by water, as the marine acid air, or vitriolic acid air; both which ſeem to be compounds exactly ſimilar to that of nitrous air. I ſhall relate the experiments which led to theſe ideas in the order in which I made them.

I began with putting ſpirit of ſalt upon manganeſe, and then diſtilling it, as Mr. Fabroni had directed me; when the firſt obſervation that ſtruck me, was a peculiar ſmell, exactly reſembling that which is procured by diſſolving *red lead* in the ſame acid. I then put a quantity of this diſtilled acid into a phial with a ground ſtopper, and a tube connected with it, and proceeded as I ſhould have done to expel air from any other ſubſtance, with the flame of a candle, receiving the produce in quick-ſilver. On the application of heat, in theſe circumſtances, it was eaſy to perceive that air, or vapour, was expelled; but it was inſtantly ſeized by the quick-ſilver, and formed a black cruſt.

Examining the air that was lodged at the top of the phial, and conſequently had been mixed with this acid vapour, I found it very little, if at all, injured. This was owing to there being little or no phlogiſton combined with the vapour, or ſeparable from it.

I then fully impregnated a quantity of ſpirit of ſalt with manganeſe, by confining them together in the ſame phial; and I afterwards endeavoured to expel air from the acid thus altered. But ſtill the vapour that came over immediately united with the quickſilver, and made a kind of amalgam with it, which, when dry, was a whitiſh or grey powder. The common air within the phial was not injured in this caſe, any more than in the former.

The above-mentioned powdery ſubſtance, being expoſed to the heat of a candle on a piece of thin glaſs, evaporated in white fumes, but left behind it a ſmall quantity of reddiſh matter, not very unlike red precipitate; which is another reſemblance between the marine acid thus altered and ſpirit of nitre. After expoſing this red matter for ſome time to a moderate heat, it became white, and ſublimed without any ſenſible change. When it was expoſed to the focus of a burning lens, upon quickſilver, it yielded no ſenſible quantity of air. I had imagined that, at leaſt during the preſence of heat, the acid, which was latent in this white ſubſtance, might have aſſumed the form of air, but I was diſappointed in that expectation.

The marine acid impregnated with manganeſe having the very ſame *ſmell* with this acid impregnated with red lead, I was led to repeat the preceding experiments with this ſubſtance alſo, and I had the ſame general reſults. For the vapour emitted by it inſtantly united with quickſilver, and formed with it a white powdery ſubſtance, of which, with a proper apparatus, I collected a conſiderable quantity; and I intend to ſubject it to various trials, of which I may poſſibly give ſome account hereafter.

A few miſcellaneous obſervations relating to the *marine acid* ſhall conclude this ſection.

The black flakes, which remain after the ſolution of ſilver in ſpirit of nitre, are diſſolved by ſpirit of ſalt, and impart to it a yellow colour.

A ſolution of iron in ſtrong ſpirit of ſalt was colourleſs; but being expoſed to the air it became brown, beginning from the top; owing, I ſuppoſe, to the precipitation of the calx of the iron, as the acid and phlogiſton eſcaped.

I had before obſerved that the vapour of marine acid does not injure air. I had the faireſt opportunity of trying this; when I had expoſed a quantity of this acid, in a glaſs tube hermetically ſealed, during ſeveral months, to a ſand heat. For the air within this tube, being examined ſeveral months after it had been expoſed to that heat, was found to be not at all injured. The air which had been confined along with vinegar in the ſame manner, and the ſame ſpace of time, was ſo far injured, that, with an equal quantity of nitrous air, the meaſures of the teſt were 1.44.

As a mixture of the nitrous and marine acid makes *aqua regia*, which diſſolves gold, I had thought it might be poſſible, that common ſpirit of ſalt, after diſſolving ſome of the nitrated calces above-mentioned, might have the ſame property; but it had not. It is now pretty well confirmed, that it is the marine acid alone, in the compoſition of aqua regia, that diſſolves the gold; this acid being dephlogiſticated by the ſpirit of nitre, which has a ſtronger affinity with phlogiſton than the marine acid has.

## SECTION XXVII.

### *An inveſtigation of the* LATERAL EXPLOSION, *and of the electricity communicated to the electric circuit in a diſcharge, from the Philoſophical Tranſactions*, vol. 60, p. 192.

SEVERAL years before I made any experiments in electricity except with a view to amuſe myſelf and my friends, I had obſerved that, in diſcharging jars, particularly ſuch as were filled with water, without any coating on the outſide, I felt a ſlight ſhock; though it was plain that the hand in which I held

I held the diſcharging rod made no part of the circuit.

Mr. Wilſon alſo, in his firſt experiments on the Leyden phial, obſerved that bodies placed without the electric circuit would be affected with the ſhock, if they were only in contact with any part of it, or very near to it. Analogous to this was his obſervation, that, if the circuit was not made of metals, or other very good conductors, the perſon who laid hold of them, in order to perform the experiment, felt a conſiderable ſhock in that arm which was in contact with the circuit. See *Hiſtory of Electricity*, p. 95.

Laſtly, in the courſe of my experiments with large electrical batteries, I found the force of this *lateral exploſion* (as I ſhall chuſe to call it) to be very conſiderable. For I ſeveral times obſerved that a chain communicating with the outſide of the battery, but which made no part of the circuit, made a black ſtain on a piece of white paper on which it accidentally lay, almoſt as deep as the chain that formed the circuit (*Hiſtory*, p. 644) and when, in order to judge, by my feeling, of the *lateral force of electrical exploſions*, I made it paſs over a part of my naked arm, the hairs of the ſkin were all ſinged, and the *papillæ pyramidales* raiſed, not only along the path of the exploſion, but alſo where ever any part of the chain had touched it, though not in the circuit. *Ib.* p. 686.

It was to aſcertain the nature and effects of this lateral exploſion, that the following experiments were made; and in reciting them I ſhall diſtinctly note the progreſs of my own thoughts in the courſe of the inveſtigation, from a ſtate of abſolute uncertainty, to that of the fulleſt ſatisfaction; and I flatter myſelf that ſome of the facts I ſhall exhibit will give pleaſure to thoſe who are beſt acquainted with, and moſt intereſted in the hiſtory of electricity.

Not having the leaſt doubt but that, if any electric ſpark paſſed between a body that was inſulated and another, the inſulated body would appear either to have received or to have loſt electricity, I imagined that nothing more was to be done than to inſulate bodies placed within the influence of the electric circuit, with pith balls hanging from them, and upon their diverging with the electric ſpark, immediately to obſerve of what kind the electricity they had contracted was; and, previous to the experiment, I conjectured it would be negative; ſuppoſing that the diſcharge from the inſide coating in an interrupted circuit was not able to ſupply the outſide faſt enough. And ſince the larger the inſulated body was, the greater the quantity of the electric fluid it was capable of receiving, or parting with, and conſequently the more ſenſible the effect would be, I began with ſuſpending on ſilken ſtrings a paſteboard tube covered with tin foil, ſeven feet long, and four inches thick, with large knobs at each end; and a braſs ball at the end of an iron rod, which communicated with the outſide of the jar, was placed within about a quarter of an inch of it, while the diſcharge was made through an inſulated interrupted circuit, no part of which was leſs than two feet from the inſulated tube.

On making the exploſion, the ſpark appeared as I expected, but, to my great ſurprize, I could not find that either poſitive or negative electricity was communicated to the inſulated tube. Neither the pith balls, nor the fineſt threads, diverged or moved in the leaſt, at or after the diſcharge, though, every thing elſe remaining in the ſame ſtate, the leaſt ſenſible electricity communicated to this tube (a quantity ſo ſmall as hardly to be viſible in the form of a ſpark at the time of communication) made the balls and the threads ſeparate to a great diſtance, and would have kept them in a ſtate of divergency more than an hour. Leſt a ſmall degree of motion, or divergency, ſhould eſcape my notice, while I was intent upon making the diſcharge, I had an aſſiſtant along with me, whoſe eye was upon the threads all the time that I was making the experiment.

This experiment, as will eaſily be imagined, ſhook my whole hypotheſis, and confounded all my ideas. I could not queſtion the fact, having repeated the experiment with preciſely the ſame event, I believe, above fifty times, on account of my having been hardly able to believe my own ſenſes, or thoſe of others.

There was an evident electric ſpark, ſometimes near half an inch in length, between the bodies compoſing the electric circuit and the inſulated tube, in ſuch a ſtate of the air, as I knew, by frequent trials, would have kept it electrified a long time; and yet there was no communication of electricity.

I do not remember that I was ever more puzzled with any appearance in nature than I was with this; and various were the ſchemes that occurred to me of accounting for it, and the methods which I propoſed to diverſify it, in order to find out the cauſe of this ſtrange phænomenon. Accordingly, I was no ſooner at liberty to attend to this experiment, but, repeating it with ſome difference in the diſpoſition of the apparatus, I obſerved that, upon every diſcharge, a ſlight motion was given to the threads that hung from the inſulated tube. Upon this, the impoſſibility of an electric ſpark neither *giving* nor *taking* any thing from an inſulated body, contrary to my attentive obſervation, and that of my aſſiſtant, I concluded that ſome motion muſt have been given to the threads before; eſpecially when I found that, in theſe latter experiments, the communicated electricity was always poſitive, the ſame with that of the inſide of the jar. But the quantity of it was ſo ſmall, that the moſt exquiſite contrivance was neceſſary to aſcertain the nature of it. For though, upon this occaſion, the lateral ſpark was near a quarter of an inch in length, the threads on the inſulated tube could only be made, by the exploſion, to change their poſition from leaning a little one way to leaning as much the other, in the neighbourhood of an inſulated braſs rod, loaded with a ſmall quantity of poſitive or negative electricity.

I could not help, however, being ſurprized that ſo large a ſpark ſhould give no more electricity to the inſulated tube than it appeared to have done; when, in other circumſtances, a ſpark ten times leſs than this would have made a great and permanent alteration: Yet, improbable as theſe circumſtances were, I entertained no doubt at that time but that theſe inſulated bodies were electrified, either poſitively or negatively, according as the inſide of the jar was poſitive or negative, by this lateral exploſion; though the *degree* was exceedingly ſmall; and I continued in this perſuaſion the longer, as it happened to be a conſiderable time before I had got another ſpark that communicated no ſenſible electricity.

I cannot help taking notice that, if it had happened that, in my firſt experiment, the inſulated tube had always acquired or loſt the leaſt ſenſible electricity (and that I afterwards found there were many chances againſt the firſt reſult) I ſhould have formed, and have acquieſced in ſome ſort of hypotheſis to account for the giving or receiving of electricity in thoſe circumſtances, and there the buſineſs would have ended. But the ſeeming contrariety of theſe appearances obliged me to purſue them farther.

Not being able completely to ſatisfy myſelf with my laſt concluſion, attended with the difficulties above mentioned, I kept diverſifying the experiments, and introducing every circumſtance that I could imagine might poſſibly affect the reſult of them; and among the reſt I made the following experiment,

ment, which quite unhinged me again, and left me as much at a loſs as ever I had been before.

Having ſuſpended a fine thread on an inſulated braſs rod, placed about one eighth of an inch from another rod, which was likewiſe inſulated, and one end of which was in contact with the coating of the jar, and having electrified the rod which ſupported the pith balls, and placed a rod loaded with the ſame electricity near them; I obſerved that, upon every diſcharge, the balls which before were repelled were inſtantly attracted by the electrified rod; and that the reſult was invariably the ſame, whether they and the rod were loaded with poſitive or negative electricity; and alſo whether the jar was charged poſitively or negatively. I repeated the experiment ſeveral hours without the leaſt variation in the event, which clearly proved that, in theſe circumſtances, the electricity of the rod that received the lateral exploſion was diſcharged by it.

Afterwards I repeated the experiment with ſome little variety, and found the electricity of the rod only leſſened by the lateral exploſion. Theſe experiments, however, by no means favoured the ſuppoſition of the uniform communication of electricity, either that of the inſide, or that of the outſide of the jar; and, together with the former experiments, convinced me that this lateral ſpark by no means produced the effect that might have been expected in communicating electricity. But with the next ſet of experiments the difficulty began a little to clear up, and it continued to do ſo gradually, till I had gained all the ſatisfaction I could wiſh for with reſpect to this puzzling phænomenon.

The firſt time that I was able to vary the electricity of the inſulated body placed near the electric circuit, or of the bodies that formed the circuit, which I now began to attend to, by any different adjuſtment of the apparatus, was on the following occaſion.

Near to an iron rod that touched the bottom of a jar charged poſitively, I placed another inſulated rod, with a pair of pith balls hanging to it, and obſerved, that when I attempted to make the diſcharge through an imperfectly conducting circuit (bringing, *e.g.* part of the table into it) a ſtrong ſpark paſſed between the inſulated rod and the other that touched the jar; and immediately the balls ſeparated as far as they poſſibly could, and continuing in a repulſive ſtate, appeared to be electrified negatively. But immediately compleating the circuit with good conductors, and making the remainder of the exploſion in a full ſpark, another ſpark paſſed between the two rods, and immediately the balls fell cloſe together again, and ſometimes would ſeparate with the oppoſite, *i. e.* poſitive electricity.

I could not, upon this occaſion, make the lateral ſpark in the full exploſion ſo great as in the imperfect diſcharge. I alſo obſerved that the more interrupted the circuit was, the farther would the lateral exploſion reach; and that the electricity which the full exploſion communicated was always poſitive when the jar was charged poſitively, and negative when it was charged negatively. The reſult of an imperfect diſcharge was always the reverſe.

Inſulating ſeveral bodies, and the jar too, charged poſitively, they all equally contracted poſitive electricity by the diſcharge.

In this ſtate of the experiments, I had no idea of the poſſibility of the lateral ſpark not communicating electricity to the inſulated body; but I imagined that the kind of electricity communicated depended upon ſome circumſtance in the diſpoſition of the apparatus that I was not ſufficiently aware of.

At length, recollecting that this laſt experiment reſembled, in ſome reſpects, that curious one of profeſſor Richman, mentioned in the *Hiſtory of electricity*, p. 272, (in which it appeared that, when the coating of either ſide of a plate of glaſs communicated with the ground, the oppoſite electricity of the other ſide was more vigorous) I was ſatisfied that the negative electricity of the bodies that formed the circuit in the imperfect diſcharge was produced by the greater difficulty with which the outſide of the jar was ſupplied, than the inſide was diſcharged, ſo that the outſide was comparatively in a ſtate of inſulation, and therefore would communicate negative electricity to all bodies within its reach; and from this I was led to conclude that, provided the jar was inſulated, and the inſide was made to part with its electricity with more difficulty than the outſide received it, the bodies that formed the circuit would contract poſitive electricity, and the reſult anſwered exactly to my expectations.

I alſo concluded that, making the interruption in the middle of the circuit, ſince, in this caſe the inſide would give, and the outſide receive with equal difficulty, the bodies in the circuit, placed between the place of interruption and the inſide of the jar, would be charged poſitively, and thoſe placed between the place of interruption and the outſide would be charged negatively; and this alſo was verified by experiment.

In this ſtate of things, I found that I could give the inſulated circuit what kind of electricity I pleaſed, provided there was any kind of interruption in ſome part of the circuit; and conjecturing that the electricity of bodies placed near the circuit would be the ſame with that of the bodies which compoſed it, I ſometimes placed the rod which ſupported the pith balls near the circuit, and ſometimes introduced it into the circuit, and found that, in both caſes, it contracted the ſame electricity. This tended to confirm me in my ſuppoſition, that the lateral exploſion was always attended with a giving or receiving of electricity, according to the nature of the circuit, and the place where it was ſituated; and I again overlooked the diſproportion between the cauſe and the effect.

Preſently after this, it occurred to me, that what may be called the redundant electricity of the outſide or inſide of the jar, ſeparate from that which is *in* the glaſs, and conſtitutes the charge, muſt have ſome concern in this event, and the ſuppoſition was verified by fact. For inſulating a jar charged poſitively, I obſerved that, when I touched the outſide coating laſt (as is commonly done in ſetting it down) and made the diſcharge through good conductors, they were all electrified poſitively; and bodies placed near the circuit were the ſame. On the contrary, if, after placing the jar upon the ſtand, I touched ed the knob of the wire, communicating with the inſide, ſo as to take off all the redundant electricity, both the circuit and the neighbouring bodies contracted negative electricity.

I had at this time quite forgot that Epinus had made the ſame obſervation, on diſcharging a plate of air, mentioned in the *Hiſtory of electricity*, p. 273; but conſidering what he ſays on that ſubject, I find he was miſtaken with reſpect to the *reaſon* of this experiment not ſucceeding with Dr. Franklin and others, who had always aſſerted, that the electric circuit contracts no electricity at all by a diſcharge. For he ſays that the ſurfaces with which the Doctor tried the experiment were not large enough to make the effect ſenſible, and that the diſtance of the metal plates was likewiſe too ſmall, as he ſays it neceſſarily muſt be, in the charging of glaſs; whereas I could give the inſulated circuit as ſenſible an electricity with a common jar as he could with his plate of air; and much more depends upon the *height* of the charge, which muſt have been inconſiderable in the plate of air, than the quantity of ſurface; which, however, may be increaſed at pleaſure by multiplying jars in batteries.

I found, however, afterwards, that much depended upon the *quantity of ſurface* in the coating, and the bodies connected with them, as containing more of that redundant electricity, the effect of which was ſeen in the laſt-mentioned experiment. For when I diſcharged the jar ſtanding in contact with the prime conductor, the tendency to the communication of poſitive electricity was ſo great, that, in that ſituation, the inſulated circuit contracted ſtrong poſitive electricity, when, every thing elſe remaining the ſame, except removing it from the conductor, and then making the diſcharge, it contracted no electricity at all.

Being now perfectly maſter of the electricity of the circuit in electrical exploſions, and being able, in two different methods, to give which of the two electricities I pleaſed, I imagined that if I could ſo balance them, as to communicate neither, there would be no lateral ſpark, as in the above-mentioned experiments. But in this I was abſolutely miſtaken.

For, in the firſt place, when after ſetting the charged jar upon the ſtand, I took off, as near as I could gueſs, one half of the redundant electricity of the inſide, and left both ſides equally electrified (as appeared by the equal attraction of the pith balls to them both) the diſcharge of the jar, through a circuit of good conductors, did not, indeed, communicate the leaſt ſenſible electricity to the circuit; but the lateral exploſion was almoſt as manifeſt as before. The pith balls, hung upon the rod that received it, never ſeparated.

In the next place I repeated this experiment by balancing the two different methods of communicating electricity to the circuit one againſt the other. For not inſulating the jar, but ſetting it upon the table, which gave the circuit, and the bodies contiguous to it, an advantage for contracting poſitive electricity by the diſcharge; but, at the ſame time, making an interruption in the circuit, by introducing part of the table into it, which tended to give them negative electricity, I could eaſily manage it ſo, that the circuit contracted neither the one nor the other; and yet, as in the former caſe, the lateral exploſion was as conſiderable as ever. The balls never ſeparated.

To vary the experiment, I placed an inſulated braſs ball, two inches in diameter, round and ſmooth, ſo as not eaſily to part with any electricity it had got, in the place of the rod that ſupported the pith balls; and having found a ſituation in which no electricity was communicated to the circuit, I obſerved that none was communicated to it though, to all appearance, it received a ſpark of about a quarter of an inch in length. At leaſt if it had contracted any, it was ſo little as to make it very problematical, whether a pith ball, or a fine thread, was moved by it, or not; whereas, when I gave it the ſmalleſt ſenſible ſpark, in any other manner, it would attract thoſe light bodies for a long time together.

The interruption of the circuit I made uſe of in this experiment was not by means of any part of the table, but only about a yard of braſs chain introduced into it, and diſpoſed between the inſide jar, and that part of the circuit near which the inſulated ball was placed. N. B. The ball muſt not be placed near the jar itſelf. For, in that ſuluation, I found that, though it was very ſmooth, and perfectly ſpherical, yet it could not be placed very near the outſide of the jar, ſtanding on the table, without contracting negative electricity in a very ſmall ſpace of time.

Theſe experiments threw me back into my former ſtate of perplexity with reſpect to the lateral ſpark; ſince, when the two electricities of the circuit were exactly balanced, it was very little diminiſhed, and yet the body that received it was not in the leaſt ſenſibly electrified. But, upon reflection, I concluded, that this lateral ſpark muſt be of the nature of an exploſion, and conſequently that an electric ſpark muſt enter and paſs out again, within ſo ſhort a ſpace of time, as not to be diſtinguiſhed, and leave no ſenſible effect whatever. For though, in this caſe, part of the electric matter natural to the body muſt be repelled, to make room for the foreign electricity, its reſtoration to its natural ſtate was ſo quick, that no other motion could correſpond to it.

This hypotheſis is favoured by the obſervation, that it is the very ſame thing whether a body be introduced into the circuit, or placed near it, with reſpect to contracting electricity, that is, whether the electric charge enter the body at one place and go out at another, or whether it be received or emitted at the ſame place.

This lateral exploſion is an effect ſimilar to a partial circuit, in which part of the electric matter that forms the charge in an exploſion goes one way, while the reſt of the charge goes another. The only difference is, that this detached part of the charge leaves the common track, and returns to it again, in the very ſame place.

Several remarkable partial circuits occurred in the courſe of my experiments before, particularly one mentioned in the *Hiſtory of Electricity*, p. 692, in which part only of the exploſion paſſed in the ſhorteſt way, while another part of it took a circuit conſiſting of the ſame materials thirty times as long, and another mentioned, p. 691, where one circuit was made through a thick rod of metal, and another, at the ſame time through the open air.

That there is an admiſſion and an expulſion of the electric matter in this lateral exploſion, ſeems evident from this circumſtance, that it is far more conſiderable when the body that receives it is large, than when it is ſmall. In the former caſe, there is room for the electric matter natural to the body to retire upon the admiſſion of the foreign electricity belonging to the charge, whereas, in the latter caſe, there is not room for it. When I placed a ſmall braſs ball, of about a quarter of an inch in diameter, near the circuit, I could not perceive that it was at all affected by any lateral exploſion; and the ſpark was very inconſiderable when I placed a needle, about two inches in length, to receive it. But when I connected the large tube above mentioned, by means of a pretty thick iron wire, to any body whatever that was placed in the neighbourhood of the circuit, I have, with a jar of only half a ſquare foot of coated glaſs, made the lateral exploſion an inch or more in length, conſiſting of a very full and bright ſpark of electric fire. Inſulated bodies of about eight or nine feet in length, ſeem to admit as large a lateral exploſion as any body whatever is capable of. For, connecting them with the earth, by means of the beſt conductors, which gave the electric matter in the bodies the freeſt receſs poſſible, I could never make this exploſion much more conſiderable, uſing the ſame jar, and all other circumſtances the ſame.

It is a manifeſt advantage in theſe experiments, that the lateral exploſion be not taken from the coating of the jar itſelf, or from any part of the circuit very near to it. I have found that, *cæteris paribus*, it is the moſt conſiderable when it is taken at the extremity of a braſs rod of one foot, or a foot and a half long, the other end of which is contiguous to the jar. It is analogous to this, that the longeſt ſpark is taken not from the body of the prime conductor itſelf, but at the extremity of a long rod inſerted into it. The electric matter ſeems to acquire a kind of *impetus* by the length of the medium through which it paſſes. But I found that the *maximum* in this caſe, did not exceed, or rather did not quite reach, three feet; for, making uſe of a thick iron rod, eight or nine feet long, the lateral exploſion, taken at the extremity of it, was about the ſame as when it was taken at the end of a rod four inches from the jar, and not half ſo conſiderable as when taken at the extremity of a rod one foot long. This I imagined might be owing to the obſtruction which the electric fluid meets with in paſſing even through metals, which appears, by my former experiments, to be much more conſiderable than was generally imagined.

Upon the whole, this remarkable experiment ſeems to be made to the moſt advantage in the following circumſtances. Let the jar ſtand upon the table; let a thick braſs rod, inſulated, ſtand contiguous to the coating;

ing; and near the extremity of this rod, place the body that is to receive the exploſion. This body muſt be ſix or ſeven feet in length, and perhaps ſome inches in thickneſs, or be connected with a body of thoſe dimenſions. Laſtly, let the exploſion be made with the diſcharging rod reſting upon the table, cloſe to a chain, the extremity of which reaches within about an inch and a half of the coating of the jar. In this caſe, the operator will hardly fail of getting a lateral exploſion of an inch in length, which ſhall enter and leave the inſulated body, without making any ſenſible alteration in the electricity natural to it.

With large jars, containing three or four ſquare feet of coated glaſs, bearing a very high charge, I make no doubt, but that this experiment might be made to much more advantage. But at the time that I was engaged in this inveſtigation I happened not to have any ſuch jar, and therefore only uſed one that contained half a ſquare foot of coated glaſs.

If the interruption in the circuit, which is almoſt neceſſary in theſe experiments, be made by introducing a length of chain into it, rather than by making part of the exploſion paſs along the table there is a medium in the length of chain, that anſwers better than either a longer or ſhorter circuit. In a long interrupted circuit, the electric matter ſeems to loſe the *impetus* which it diſcovers in a ſhort one.

In all theſe caſes, the electric charge ſeems to remain for a moment in the parts of the interrupted circuit, and therefore inſtantaneouſly ruſhes, in all directions, as well towards bodies that are not placed along its paſſage to the jar, as thoſe that are; but, when the ſame charge occupies a larger circuit, it has more room to expand itſelf, and is not ſo ſtrongly impelled to deſert it. I found, however, by repeated trials, that when I made uſe of three yards of braſs chain in the circuit, there was a diſtance to which the lateral exploſion would not reach. The ſame diſtance it alſo would not reach when the circuit conſiſted of only one braſs rod; but it reached it with great eaſe when only half a yard of chain was uſed, even without any other interruption in the circuit. But it reached to a much greater diſtance when the chain was very ſhort, and the interruption was greater in other reſpects.

I had imagined, that, ſince the body which had received the lateral exploſion, contained, for a moment, more than its natural quantity, that, if it were acutely pointed, ſome would eſcape, and that, upon the return of the exploſion, the body would be exhauſted. But I found no ſuch effect, though I affixed fine needles to the bodies I made uſe of. The lighteſt pith balls, placed near the extremities of theſe needles, were not in the leaſt affected by the exploſion.

When I placed a number of braſs balls, one behind another, the lateral exploſion paſſed through them all; being viſible in in the intervals between each of them, and returned the ſame way, leaving them all in the ſame ſtate in which it found them; and a great number of lateral exploſions might be taken at the ſame time, in different parts of the circuit, ſome of them very near one another.

It made no difference whether the lateral exploſion was received on a flat ſmooth ſurface, or the points of fine needles. In both caſes, the ſpark was equally long, and vivid.

I had no ſooner completed theſe experiments on the lateral exploſion, but I had a curioſity to ſee what kind of appearance it would make *in vacuo*, ſince no other phænomenon in electricity reſembles it. In all other caſes, the electric matter ruſhes in one ſingle direction; whereas, in this, it goes and returns in the ſame path; and, as far as can be diſtinguiſhed, in the ſame inſtant of time; ſo that all the difference of the two electricities, which are ſo conſpicuous in vacuo, muſt here be confounded. Accordingly, I found, though my pump was not in good order, that I could perceive this exploſion in vacuo at the ends of rods placed ſeveral inches aſunder; and when they brought within about two inches, they ſeemed to be joined by a thin blue or purple light, quite uniform in its appearance. As theſe rods were made to approach, this light grew denſer, but ſtill exhibited no ſuch variety as is obſerved between the bodies that *give* and *receive* electricity, in the common experiments in vacuo.

I was pretty ſoon convinced, that uncoated jars could not be uſed to any more advantage in theſe experiments than thoſe that were coated; ſince the want of coating only operated as an interruption in the circuit, occaſioning a difficulty in the admiſſion of the charge on the outſide of the jar. And, in all caſes, the greater this difficulty of paſſage was made, provided the diſcharge was made at once, the more conſiderable was the lateral exploſion; and the greater ſhock was given to the hand that held the diſcharging rod, which ſhock was nothing more than one of theſe lateral exploſions, iſſuing from the rod, as part of the circuit.

I ſhall conclude the account of theſe experiments with obſerving, that they may, poſſibly, be of ſome uſe in meaſuring the conducting power of different ſubſtances;

ſince, the greater is the interruption in the electric circuit, occaſioned by the badneſs of its conducting power, the more conſiderable, cæteris paribus, is the lateral exploſion.

## SECTION XXVIII.

### *Miſcellaneous experiments in electricity.*

### 1. *Experiments relating to the breaking of glaſs jars by electric exploſions.*

FEW perſons, I believe, have had ſo much experience in jars broken by electric exploſions, as myſelf; having originally conſtructed very large batteries of very thin glaſs. In the hiſtory of my electrical experiments, I have mentioned an inſtance of ſix jars, containing each one foot of coated ſurface, burſting at one exploſion. Since that time, having diſcharged a battery at Leeds, in the preſence of Dr. Franklin and Mr. Canton, though I perceived nothing particular, and ſuſpected no accident, at the time (the full force of the exploſion having, to all appearance, been received in the uſual manner) yet when I was about to charge it again, I found that no leſs than *ten* jars had been broken at that one time. It was ſome conſolation to me, however, that the accident happened in the preſence of two ſuch eminent electricians.

Having been ſo great a ſufferer in this way, and having always ſuch a number of broken jars at hand, I could not be without thinking of expedients to repair them; and nothing *a priori* promiſes ſo well to anſwer the purpoſe, as ſuch *cements* and *varniſhes*, as are known to be impermeable to the electric fluid. But though theſe cements and varniſhes will ſometimes anſwer pretty well with *thick* jars, I have not yet found any method of effectually repairing *thin* ones, ſuch as mine have generally been; and I do not know of any facts that ever puzzled me more than the following, which occurred in my attempts to repair them.

I have found invariably, ſo that, extraordinary as it may ſeem, it is impoſſible that I can be miſtaken with reſpect to it, that whenever I had covered the fractured place of a jar with any kind of cement, or varniſh, (and particularly an excellent *amber varniſh*, recommended to me for that purpoſe by my then tutor in chemiſtry, Mr. Turner of Liverpool, and which hardened into a ſubſtance as firm as glaſs) the jar never failed to break again at the very next charge, and generally before it had received half its proper charge. But what is moſt remarkable, is the following circumſtance, that the new fracture was never made in the place of the old one, but always exactly in ſome place where the cement terminated. There I found a new perforation of the glaſs, and a new fracture, which had no communication with the former. To whatever diſtance I extended this new coating of cement, the event was the very ſame. For the fracture never failed to happen, and exactly in ſome part of the glaſs, where the cement ended.

Seeing in ſo many inſtances, that the new fractures had no connection with the old ones, but that they always happened at the termination of the cement, I concluded that the coating of cement muſt have been one principal cauſe of the fracture; and therefore I repeated the experiment with a jar that had *not* been broken. And, in order to be quite ſure that the expected fracture, if it ſhould happen, might appear to be cauſed by the coating of cement only, I took a phial, and coating it inſide and outſide, in the uſual way, I found that it bore a full charge very well. For my greater ſatisfaction, I charged it and diſcharged it ſeveral times.

Having in this manner aſcertained the ſtrength of the jar, I took off a little of the outſide coating, and put on it a ſmall patch of cement, about an inch in diameter. Then, drawing the former coating over it, I proceeded to charge the phial as before. But before it had received half its full charge, it burſt by a ſpontaneous exploſion, not indeed at the termination of the cement, as in all the caſes above referred to, but in the middle of the patch, where it happened to be exceedingly thin, much thinner than near the extremity of the patch.

I then covered another phial entirely with cement, and after coating it inſide and outſide, in the uſual way, proceeded to charge it as before; when this phial likewiſe burſt, and in a place where neither the glaſs nor the cement was particularly thin; the cement being of the thickneſs of a ſhilling, and the fracture happening near the bottom of the phial; where the glaſs is generally pretty thick.

Laſtly, I covered a phial both inſide and outſide with cement, and after that coated it inſide and outſide with metal, in the uſual way; ſo that all the glaſs ſeemed to be guarded from any accident. But, notwithſtanding this, it burſt at the very firſt attempt to charge it.

I expected that, by covering the whole phial with cement, ſo that there was no place of termination, where the new fractures had generally happened, the jar would be more ſecure. But I found to my coſt, that even an entire coating of cement was no more ſafe than a partial one. Why a glaſs jar ſhould be endangered by an *electric coating*, which cement is, I cannot conjecture; but I propoſe, at my leiſure, to diverſify this experiment, and ininveſtigate the cauſe of it, if I can.

## 2. *Of the ſuppoſed nonconducting powers of water and quickſilver, in the ſtate of vapour.*

In my laſt publication I gave an account of ſome experiments, which ſeemed to prove that ſteam, or the proper vapour of water, and even of quickſilver, were nonconductors of electricity; becauſe the electric matter paſſed through them both in a *full ſpark*, exactly as it does in air, which is known to be a nonconductor. I concluded that, had the electric matter paſſed in the ſubſtance of the vapour itſelf, ſo as to be properly conducted by it, it would have paſſed inviſibly, as it does in metals or water, &c. But in repeating theſe experiments with ſome variation, I have ſince been led to conclude, that, though the electric matter does paſs through theſe vapours, to all appearance, exactly as it does through a body of *air*, yet they are not capable of confining the electric matter, ſo as to inſulate electrified bodies, as air is.

Theſe vapours may, however, in reality, be nonconductors of electricity, and it may be owing to the *heat*, which is neceſſary to the preſerving their form of vapour, that they do not inſulate. For I found that, in the ſame degree of heat, even the glaſs which contained them would not inſulate, and that it was pervious to the electric exploſion, without any injury to it. Though the experiments are, therefore, inconcluſive, the probability *a priori*, is ſtill, I think, in favour of the opinion, that every ſubſtance in the form of air is, when cold, a nonconductor, as air itſelf is.

Beginning where I had left off, I filled a glaſs ſyphon with mercury, and putting each of the legs into ſeparate glaſs cups of mercury, I placed the upper, or the bent part of the ſyphon, near the mouth of a ſmall furnace, that the mercury in that place might be turned into vapour, and the running mercury might deſcend into the legs of the ſyphon. Then applying the rod of a charged jar to a braſs ball, connected with the mercury in one of the glaſs cups, I found that it could not retain electricity the ſmalleſt ſpace of time, though the mercury was completely inſulated; ſo that the electric matter which I communicated to it muſt have paſſed both through the vapour and the glaſs itſelf. In this caſe, however, the electric matter was tranſmitted inviſibly. But when I made this ſyphon part of an electric circuit, the electric matter paſſed viſibly through the vapour, in the manner deſcribed before.

I then introduced only one leg of the ſyphon into the circuit, and the exploſion paſſed freely through the vapour, and the glaſs, at the place where it was hotteſt. It was viſible in the vapour, but divided into ſeveral ſtreams towards the top of the glaſs, and then paſſed inviſibly through the hot fire, the ſpace of ſeveral inches. That it did take this road, was evident by making interruptions in the circuit, which conſiſted of pieces of metal lying on the floor, beyond the fire. But when the circuit was much interrupted, I always perceived that part of the exploſion went round to the quick-ſilver in the other leg of the ſyphon, though it was completely inſulated, and there was no paſſage for it that very way into the air. This, therefore, muſt have been a caſe of the *lateral exploſion*, which paſſes and returns at the ſame inſtant, of which I gave a particular account, in a paper inſerted in the *Philoſophical Tranſactions*, and which is contained in the laſt ſection.

Laying aſide the ſyphon, I filled a glaſs tube, cloſed at one end, with mercury, and then inverting it, and heating the upper part of it, while the lower part was plunged in a glaſs cup of mercury, I had the ſame reſults as with the ſyphon; the mercury, ſo inſulated, not being capable of retaining electricity, and the exploſion paſſing quite freely through the vapour and the glaſs itſelf.

When I placed a braſs rod very near the top of the tube, and made it part of the circuit, I found that the exploſion paſſed through the vapour, and the ſubſtance of the glaſs, to come at it; but I could not perceive any ſpark between the glaſs and the rod, though the electric matter paſſed viſibly through the vapour within the glaſs. This ſeems to ſhew that ſomething within the fire, in the ſpace between the glaſs tube and the braſs rod, was a proper conductor of electricity, as it paſſed inviſibly in that place. But that the electric matter found ſome reſiſtance within the glaſs, is, I think, evident, from it paſſing viſibly there, juſt as it does in all kinds of air.

## SECTION XXIX.

### *Of ſound in different kinds of air.*

ALMOST all the experiments that have hitherto been made relating to *ſound*, have been made in common air, of which it is known to be a vibration, though it is likewiſe known to be capable of being tranſmitted by other ſubſtances. There could be little doubt, however, of the poſſibility of ſound *originating* in any other kind of air, as well as of being *tranſmitted* by them; but the trial had not been actually made, and I had an eaſy opportunity of making it.

Beſides, the experiments promiſed to aſcertain whether the *intenſity* of ſound was affected by any other property of the air in which it was made than the mere *denſity* of it. For the different kinds of air in which I was able to make the ſame ſound, beſides differing in ſpecific gravity, have likewiſe other remarkable chemical differences, the influence of which with reſpect to ſound would, at the ſame time, be ſubmitted to examination.

Being provided with a piece of clock-work, in which was a bell, and a hammer to ſtrike upon it (which I could cover with a receiver, and which, when it was properly covered up, I could ſet in motion by the preſſure of a braſs rod, going through a collar of leathers) I placed it on ſome ſoft paper on a transfer. Then taking a receiver, the top of which was cloſed with a plate of braſs, through which the braſs rod and collar of leathers was inſerted, I placed the whole on the plate of an air pump, and exhauſted the receiver of all the air that it contained. Then removing this exhauſted receiver, containing the piece of clock-work, I filled it with ſome of thoſe kinds of air that are capable of being confined by water, by means of a bent glaſs tube inſerted into a piece of braſs, which I could ſcrew into the bottom of the transfer, ſo as to introduce the bended tube, through the water of my trough, into a jar containing the air on which I wiſhed to make the experiment. For a deſcription of this apparatus, ſee vol. i. pl. 2. fig. 14.

When this was done, I removed the glaſs tube, and then I had the receiver filled with that ſpecies of air in which I wiſhed to produce the ſound, and the apparatus for making the ſound within it. Then by forcing down the braſs rod through the collar of leathers, I made the hammer ſtrike the bell, which it would do more than a dozen times after each preſſure. And the inſtrument was contrived to do the ſame thing many times ſucceſſively, after being once wound up.

Every thing being thus prepared, I had nothing to do, after filling the ſame receiver with each of the kinds of air in its turn, but receding from the apparatus, while an aſſiſtant produced the ſound, to obſerve at what diſtance I could diſtinctly hear it. The reſult of all my obſervations, as far as I could judge, was that the intenſity of ſound depends ſolely upon the *denſity* of the air in which it is made, and not at all upon any chemical principle in its conſtitution.

In inflammable air the ſound of the bell was hardly to be diſtinguiſhed from the ſame in a pretty good vacuum; and this air is ten times rarer than common air.

In fixed air the ſound was much louder than in common air, ſo as to be heard about half as far again; and this air is in about the ſame proportion denſer than common air.

In dephlogiſticated air the ſound was alſo ſenſibly louder than in common air, and as I thought rather more than in the proportion of its ſuperior denſity; but of this I cannot pretend to be quite ſure.

In all theſe experiments the common ſtandard was the ſound of the ſame bell in the ſame receiver, every other circumſtance alſo being the ſame; the air only being changed, by removing the receiver from the transfer and blowing through it, &c.

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## SECTION XXX.

### *Miſcellaneous experiments.*

### 1. *Of lime water in a ſolution of iron with ſpirit of nitre.*

I HAD diſcovered (ſee vol. iv. p. 288) that the cauſe of the change of colour from blue to red in the calx of iron, is the dephlogiſtication of it; tracing the progreſs of the phlogiſton through a large body of water, at the bottom of which the precipitate of the iron lay, into the air above it, which it phlogiſticated. I then produced the blue precipitate, by pouring a diluted ſolution of fixed alkali into a ſolution of green copperas. But I have ſince accidentally met with a much better method of making the experiment, by means of lime water.

The trough in which I make my experiments on air being at one time very foul, with various metallic ſolutions, eſpecially in conſequence of having diſſolved iron in ſpirit of nitre, for the production of nitrous air, and other purpoſes, and it not having been convenient to change the water, I continued to uſe it in that ſtate; when caſually pouring a little lime water into it, I obſerved that a precipitate of a very deep blue colour was formed. It was ſo beautiful, that, having been obliged to leave my experiments, for the ſake of a ſmall excurſion to Bath, before I ſaw any farther into it, I remember telling a friend whom I met there, that I thought it poſſible that I had accidently diſcovered a new and cheap method of making Pruſſian blue. However my dream of a diſcovery vaniſhed on my return home, when I obſerved the bottom of my trough covered with a very lively *red*. But when I turned it up, I found the red was only ſuperficial, and that the precipitate underneath was of as deep a blue as ever.

I then repeated the experiment in ſmall jars, phials, &c. and was much better pleaſed with the reſult than when I had made uſe of a ſolution of alkali in order to make the precipitate. Here the lime is ſeized by the acid, as it was before by the alkali; and in both caſes the calx of the iron is ſet at liberty, and depoſited in a phlogiſticated ſtate. But it readily parts with its phlogiſton if pure air be at hand, even though ſeparated from it by a body of water, of I believe any depth. Theſe experiments ſhew that water, though capable of receiving phlogiſton, is not capable of retaining it in the preſence of air, which appears to have a much ſtronger affinity with it.

### 2. *Of an unexpected appearance of volatile alkali.*

Having, for the purpoſe of producing a large quantity of that kind of nitrous air in which a candle burns with an enlarged, or with a vivid flame, filled a large jar with pieces of iron wire, and having repeatedly poured upon them a diluted ſolution of copper in the nitrous acid, at length a thick incruſtation was formed upon them; and having no occaſion to make uſe of the jar for ſeveral months, I took no notice of it till I found the jar was burſt by the ſwelling of that ſaline incruſtation.

The ſubſtance of this matter was generally red, being the calx of iron; but there was mixed with it a quantity of *green matter*, which, when broken had a ſtrong ſmell of volatile alkali. I then doubted whether this aroſe from any of the materials I have mentioned, or from ſomething elſe which had got into the jar, unknown to me. If the former were the caſe, which, however, at that time, I could hardly ſuppoſe, I thought it to be not a little remarkable; but I have ſince had another opportunity of obſerving the ſame fact; having examined a ſecond jar filled with iron wire, which had been treated in the ſame manner, and found the ſame ſtrong ſmell of volatile alkali,

alkali. Alſo I now the leſs wonder at this fact, which puzzled me ſo much at the firſt, as I find, in Mr. Keir's very valuable notes to his tranſlation of Mr. Macquer's chemical dictionary, that volatile alkali has been found in many earthy ſubſtances, and amongſt others, in *ruſt of iron diſtilled.*

In this caſe, the calx of the iron being ſuperſaturated with phlogiſton from the nitrous air, decompoſed by it, the alkali, of which this and other metallic calces conſiſts, uniting with it, becomes volatile alkali.

It ſhould ſeem that, in general, the calces of metals contain leſs phlogiſton than the metals themſelves; and for this reaſon I was originally led to conclude, that nitrous air expoſed to iron, which is evidently turned to *ruſt*, or a *calx* in it, had received phlogiſton from the metal; and I therefore termed the nitrous air that had been ſo treated *phlogiſticated nitrous air.* I now think it moſt probable that this ruſt of iron contains more phlogiſton than the iron itſelf, and that the nitrous air, in which, after this proceſs, a candle burns better than in common air, is properly termed *a dephlogiſticated nitrous air*, having parted with its phlogiſton to the iron.

I expect to find, in my future experiments, that it may not be difficult to determine whether this ruſt of iron contains more or leſs phlogiſton than the iron itſelf. The former I own I ſtrongly ſuſpect, and therefore that it differs much from common ruſt of iron. For I am far from being diſpoſed to queſtion the truth of the common opinion, that metals conſiſt of phlogiſton and a peculiar earth.

### 3. *Of air not being ſenſibly injured by offenſive putrid ſubſtances.*

It has been obſerved both by myſelf and others, that air exceedingly offenſive to the noſtrils is not always properly phlogiſticated, ſo as to be diſtinguiſhed by the teſt of nitrous air. For though it may be true that phlogiſton is the thing that conſtitutes *ſmell*, or at leaſt that it is in ſome manner eſſential to it, that phlogiſton which ſenſibly affects the olfactory nerves may be attached to ſomething

thing that is only *diffuſed* through the air, and not properly *incorporated* with it. For when this air, ſo exceedingly offenſive to the noſtrils, is made to paſs through a body of water, this phlogiſton is entirely ſeparated from it, and leaves the air through which it was diffuſed, and which it had ſeemed to contaminate, quite pure and inoffenſive.

In order to make full proof of the truth of this obſervation, and alſo with the farther view, of trying whether the quantity of phlogiſton contained in an animal ſubſtance might be ſo far exhauſted by putrefying in quickſilver, as to be unable to phlogiſticate common air, I confined a large piece of the tendon of a neck of veal, and likewiſe a whole mouſe, in ſeparate veſſels of quickſilver, ſome time in September, 1779; and when they had yielded all the air that I could perceive they would yield, and of which an account has already been given in a former ſection, I took what remained of them both in the April following, and putting them into a jar of common air, containing about ſeven ounce meaſures, I examined this air after two days, and did not find it ſenſibly injured, though the ſubſtances were very offenſive to the noſtrils. After keeping them, however, in the ſame jar about two months longer, I found the air to be phlogiſticated.

Notwithſtanding this, I make no doubt but that in *length of time* theſe ſubſtances would have loſt all their power of phlogiſticating air. But whether this property, or that of yielding an offenſive ſmell, would have gone firſt, I had no opportunity of obſerving, in conſequence of removing my habitation, by which I was obliged to put an end to the proceſs. It appears, however, ſufficiently, that very much of the power of theſe putrefying ſubſtances to phlogiſticate air was gone before they ceaſed to be offenſive, though it is probable they were not ſo highly offenſive as they had been before.

# SECTION XXXI.

## *Remarks on certain paſſages in the preceding volumes of my* Obſervations on Air, *explaining, or correcting them, by the help of ſubſequent experiments and obſervations.*

N. B. The firſt number denotes the *page*, and the ſecond the *paragraph*. When no paragraph is expreſſed, the *firſt* in the page is to be underſtood.

VOLUME i. page 38, paragraph 2. Inflammable air burns blue when it is mixed with fixed air. The inflammable air, in this caſe, came, I doubt not, from the *iron*, which I afterwards found to yield inflammable air by heat only. See vol. ii, page 107.

P. 42. The addition of permanent air, in this caſe, came, I doubt not, from the iron filings and brimſtone; which, in time, even in the temperature of the atmoſphere, yields a quantity of inflammable air, which is liable

able to be changed afterwards, and often preſently, into phlogiſticated air, as will be ſeen in the preſent volume, p. 83, &c.

P. 45. If fixed air makes a part of the conſtitution of common air, it ſhould ſeem that it ought to be depoſited when brimſtone is burned over it, as well as when other ſubſtances are treated in the ſame manner. For though the acid of vitriol may unite with the lime, in the lime water over which it is burned, it would hardly make part of the ſame ſubſtance with, at leaſt, any conſiderable portion of fixed air; becauſe the ſtronger acid would expel the weaker, if it had been there before.

In ſome ſaline ſubſtances, as *alum*, there are both vitriolic acid and fixed air, but the latter is in ſmall quantity.

P. 56. More air is procured from any ſubſtance by a quick than by a ſlow proceſs, and nitrous air continuing a long time unabſorbed by water becomes leſs capable of being abſorbed, as will be ſeen in the preſent volume, p. 129. In both theſe caſes, the influence of *time* is very remarkable.

P. 57, 2. I ſuſpect that this ochre, and theſe flowers of zinc, were produced by ſome part of the ſolution of thoſe metals mixing with the water in the trough; after which the phlogiſton eſcaping into the air, the calces were precipitated; and that this ochre had never been incorporated with the air, at leaſt no more of it than was perceived to make it turbid, when it was firſt produced. But I ſtill think it probable, that ſome earth, and of courſe earth from the metals, forms the baſis of inflammable air.

P. 59, 2. It is poſſible that in pure water, inflammable air might not be changed into phlogiſticated air, though I cannot tell what kind of impregnation in the water promotes this change. Urine will do it, as may be ſeen in this volume, p. 129.

P. 61, 2. Since the experiments on the willow plant, recited in this volume, I rather think that the diminution of inflammability in this quantity of air was owing to the growth of the plant in it.

P. 65, 4. The caſe of the inflammable air firing with one exploſion in the vapour of ſpirit of nitre is ſimilar to a candle burning with a vigorous flame in nitrous air expoſed to iron, &c. It contains a vapour, or a ſpecies of air, that is capable of being abſorbed by water. This vapour, &c. is capable of taking phlogiſton from burning bodies, though it will not ſupport animal life. But they differ in this, that this nitrous vapour which inflammable air takes immediately from ſpirit of nitre, is inſtantly abſorbed by water; whereas that which is produced from nitrous air, as well as in ſome direct proceſſes, by means of nitrous acid, is capable of being transferred through water many times, and is ſo combined with ſome other principle, as not to diſcover any mark of acidity, any more than nitrous air itſelf, and in ſome caſes leſs than nitrous air.

P. 69, 2. Water will take more or leſs of inflammable air, as well as of other kinds of air, in proportion to the quantity of air which it contains already.

P. 84, 3. Six ounce meaſures of phlogiſticated air were produced in this caſe from a ſingle mouſe putrefying in water; whereas if it putrefies in quickſilver, there will not be a ſingle ounce meaſure of ſuch air procured. This is a ſubject that deſerves to be inveſtgated farther.

P. 99. As phlogiſticated air is common air loaded with phlogiſton, though fixed air be an acid, and, like other acids, has ſome affinity with phlogiſton; yet the baſis of common air (which appears to be an acid principle common to the nitrous and vitriolic acids) has a ſtronger affinity with it; and therefore it is not in the power of fixed air to deprive the common air of the phlogiſton which is incorporated with it, ſo as to improve that air.

P. 103, 2. It is moſt probable, that fixed air in the bowels ſtrengthens the tone of them, and thereby enables them to expel the putrid matter, and that it does not properly, as I imagined at firſt, unite with the putrid matter, and thus render it leſs offenſive. In this manner, too, it is probable, as Dr. Millnan informs me is his opinion, that fixed air acts in the ſtomach, as a medicine for the ſea ſcurvy.

P. 106. This ſmell of volatile ſpirit of vitriol muſt have ariſen from vitriolic acid air, produced by the union of the acid of vitriol contained in the ſulphur, with phlogiſton, coming probably from the iron.

P. 107. That inflammable air muſt have been in a ſtate of diminution by ſtanding in water. For I have ſince found that, with long ſtanding, this mixture produces air, and generally inflammable air, even in the temperature of the atmoſphere. See p. 107. of this volume.

P. 114, 2. The nitrous acid will unite with much of the lime, with which it makes a ſalt, that is perhaps in a conſiderable degree ſoluble in water. However, many experiments, and eſpecially ſome recited in this volume, make it doubtful whether there be any fixed air properly incorporated with common air, ſo as to make a conſtituent part of it.

P. 121, 2. I do not yet perfectly underſtand the nature of this filmy matter collected from my trough.

P. 126, 2. Afterwards air conſiderably nitrous was procured from lead. See vol. ii. p. 173.

P. 127. Here was an inſtance of the calx of zinc abſorbing ſpirit of nitre, as deſcribed in this volume, p. 237, &c.

P. 128, 2. In vol. 3. p. 165 and 166, it will be found that I procured different quantities of nitrous air from iron. To aſcertain this quantity with accuracy, more attention ſhould be paid to the quantity of phlogiſton both in the nitrous acid, and the iron itſelf, as ſeveral chemiſts have obſerved that this metal varies much in this reſpect. I propoſe ſome time or other to repeat theſe experiments wiih an attention to more circumſtances than I was apprized of in this early ſtage of the buſineſs.

P. 135, 2. The yellow tinge of the water over which the metals were calcined aroſe probably, from the calces of the metals, and the *ſmell* from part of the phlogiſton ſet looſe in the proceſs.

P. 137. This white powdery ſubſtance could be nothing but the calx of the metal.

P. 138, 2. It

P. 138, 2. It will appear by future experiments, that the oil contributes to phlogiſticate the air more than any other ingredient of the paint.

P. 153, 2. The marine acid air muſt have ſeized upon the water, and have left it in the ſtate of calcined alum.

P. 154, 3. The effect of the proper acetous fermentation of air deſerves to be examined with attention.

P. 155, 3. This is the firſt dephlogiſticated air that I procured. The note was written when I thought it was dephlogiſticated nitrous air that I had got.

P. 156, 2. As I never had any inſtance of dephlogiſticated air becoming thoroughly noxious, and being reſtored ſo ſoon, I ſuſpect there muſt be ſome miſtake in this place. But it is of little conſequence now, ſince the nature and properties of this kind of air are fully aſcertained.

P. 171. 4. I do not know what the white cloud mentioned in this place can have been.

P. 175, 3. I ſuſpect that the inflammability of this air came from ſome thing mixed with the alkaline air, and not from any thing eſſential to it. My reaſons will be ſeen in the preſent volume; p. 224.

P. 179. *Note. Nitrous air phlogiſticated with liver of ſulphur* ſhould be ſtruck out from this note, as the air to which I there allude appears now to be dephlogiſticated, and not phlogiſticated.

P. 186, 3. The quantity of ſolid matter formed by taking the electric ſpark over lime water was ſo ſmall, that the experiment ought to be repeated on a larger ſcale, in order to examine what kind of ſubſtance it really is.

P. 192, 2. It could not be a real, but only a ſeeming calcination, or a diſperſion of the tin, that was made in a cloſe veſſel.

P. 193, 2. Had I uſed more heat, dephlogiſticated air would have been produced, as well as fixed air.

P. 214, 3. Did not the fixed air in this place come from a ſlight tendency to putrefaction in the bladder?

P. 216, 2. The power of nitrous air after it has been expoſed to iron &c. to diminiſh common air, is explained in the preſent volume, p. 203.

P. 218, 2. The theory of this diminution of nitrous air is erroneous, as it is probable from ſubſequent obſervations, that the iron inſtead of loſing phlogiſton gains an addition of that principle from the nitrous air, which is thereby dephlogiſticated.

P. 219, 2. Afterwards I found that, after a certain period in this proceſs, a dephlogiſticated nitrous air was produced. See vol. iii, p. 140.

P. 220, 3. Having never ſince found that nitrous air, without agitation in water, is diminiſhed by freſh nitrous air, in conſequence of expoſure to iron, &c. I conclude that I muſt have made ſome miſtake with reſpect to this experiment.

P. 249, 2. This remaining air I now find comes from the iron filings and brimſtone, which firſt yields inflammable; whereas liver of ſulphur ſeldom gives any.

P. 250, 3. It will be found that when this experiment is made with more accuracy, theſe different kinds of air expand unequally with the ſame degrees of heat. See vol. iii, p. 347.

P. 252. The vapour of ether mixes with the air, and for a time aſſumes the form of air; but it is capable of being imbibed by water.

P. 254, 3. This ſubſtance containing nitre yielded dephlogiſticated air, by the help of which the ſubſtance could burn; but in vacuo the dephlogiſticated air was too much dilated, at the moment of its generation, to ſuſtain any fire.

P. 260, 2. In this place I ſuppoſed heat to conſiſt in a vibratory motion of the particles of bodies; and ſenſible heat probably does conſiſt in, or is accompanied by, ſuch a motion. But there may be a *principle of heat* latent in bodies, and not manifeſt by any ſenſible effect. Heat may therefore be what is uſually termed *a ſubſtance*, whether it have the property of *weight*, that is, whether it be ſubject to the action of gravity, or not.

P. 266, 2. The experiments recited in this volume prove that vegetation increaſes the quantity of the air which it purifies.

P. 271, 2. It is dephlogiſticated air that is formed in the accenſion of gunpowder, and which enables the other materials to burn with the violence which is peculiar to that compoſition.

Ib. 3. It appears from ſubſequent experiments, that it is not the marine acid that is the baſis of common air, but an acid principle that is common to the vitriolic and nitrous acids. But though it ſhould be a proper nitrous acid that is the baſis of common air, an earth is alſo eſſential to it; and this chemical compound of nitrous acid and earth, may have a ſtronger affinity with phlogiſton than the nitrous acid alone has, and therefore may ſeize upon it, ſo that the nitrous acid in the nitrous air may be precipitated by this means.

P. 273. It muſt be phlogiſton that is taken from nitrous air when it is expoſed to iron or liver of ſulphur, if not alſo when the electric ſpark is taken in it, becauſe it is left in a dephlogiſticated ſtate. But it may be difficult to trace the progreſs of the phlogiſton which it has loſt in all theſe caſes.

P. 279. In Mr. Elliot's writings will be found ſome very ingenious conjectures, and pretty well ſupported, concerning the manner in which it may be ſuppoſed that muſcular motion is performed, and the influence of phlogiſton in this buſineſs.

P. 281. See the remark on p. 260.

## *Remarks on the* ſecond *Volume.*

P. 8. 2. I found afterwards (ſee vol. iii. p. 360.) that vitriolic air would not diſſolve ice, and therefore I conclude that, in this caſe, a little moiſture might adhere to the ice, which, unperceived by me, might imbibe the air. I had the leſs ſuſpicion in this caſe, from having found that marine acid air, fluor acid air, and alkaline air diſſolve ice. So that the property of vitriolic acid air, *not* to diſſolve ice, is a remarkable exception to what may be called a general rule.

P. 171. I imagine that this nitrous vapour ſeized upon the phlogiſton of the nitrous air, and thereby decompoſed it, in the ſame manner as the nitrous acid itſelf will do.

P. 231, 2. It will be ſeen that a ſmall quantity of nitrous acid, from the decompoſed nitrous air, muſt have mixed with this water. See the preſent volume, p. 141.

P. 232. That alkaline air will not diſſolve copper may be owing to its being already ſaturated with phlogiſton, though when combined with water it does diſſolve this metal. This compound, of alkaline air and water, may have quite different properties from the alkaline air alone.

Ib. Cauſtic alkali may require phlogiſton to aſſume the form of air, and it may not be eaſy to find any ſubſtance that has a leſs affinity with it than this alkali. Or the alkali may have a ſtronger affinity with water than the phlogiſton.

## *Remarks on the* third *volume.*

P. 26. It is very poſſible that the water in which this experiment was made might contain ſome fixed air, which, as I have obſerved, is readily communicated to any kind of air that is made to paſs through it. See vol. iii, p. 355.

P. 42. The experiments made with red precipitate make it probable, that by far the greateſt part of the air conſiſts of ſpirit of nitre, or rather of an acid principle common to it and oil of vitriol; ſince all the mercury may be revived from red precipitate, except about one twentieth part.

P. 54, 2. The nitrous acid in the ſolution of copper from which the nitrous air is extracted is not taken into the account of this proceſs, and thereſore there may be much leſs nitrous acid in four ounce meaſures of nitrous air than in forty-two of dephlogiſticated air.

P. 102, 2. I afterwards found air in the ſtalks of ſome plants to differ in quality from the external air. See vol. iv. p. 313, &c.

P. 128, 2. It is imagined that nitrous acid combined with water has a ſtronger affinity with phlogiſton than nitrous acid in the form of vapour, becauſe it ſeizes upon the phlogiſton of nitrous air, and decompoſes it.

P. 139, 2. I have now no doubt, but that the former of theſe ſuppoſitions is the true one, or that this kind of nitrous air in which a candle burns contains a redundancy of nitrous vapour, diffuſed through the air, though ſo combined with ſome other principle, as not to ſhew any mark of acidity, when imbibed by water, &c. See this volume, p. 133, 203.

P. 146. The nitrous acid vapour muſt, I ſhould imagine, be combined with ſome portion of phlogiſton, if not with ſome other principle, not to give acidity to water.

P. 165. I have ſince found that nitrous air contains juſt the ſame quantity of phlogiſton with inflammable air, bulk for bulk. See vol. iv. p. 378.

P. 201. This copious production of nitrous from water impregnated with nitrous vapour is a fact of a very remarkable nature, and deſerves to be farther attended to.

P. 230, 4. The calx of lead, like the calces of other metals, imbibes ſpirit of nitre, and a little water. They all emit them again with heat. See the preſent volume, p. 241.

P. 267. The increaſe of the quantity of inflammable air from agitation in oil of turpentine, may be explained by my having found that oil of turpentine ſometimes contains a conſiderable quantity of inflammable air, which may be expelled by heat. See vol. 4. p. 363.

P. 298. I have ſince diſcovered that the colour of ſpirit of ſalt is owing to ſome earthy matter diſſolved in it. See vol. 4. p. 78, &c.

P. 350, 3. If, as is now ſaid to be diſcovered by profeſſor Bergman, ſpirit of wine conſiſts of fixed air combined with the acid of ſugar, this concluſion is not juſt. There are proceſſes, however, which I ſtill think prove fixed air to be a factitious thing, eſpecially the phænomena of the phlogiſtication of common air, becauſe the diminution is completed when inflammable air is fired in it, without any appearance of fixed air.

## *Remarks on the* fourth *volume.*

P. 77. The reaſon why the two quantities of air occupy leſs ſpace when they are made to paſs up the tube ſlowly, is that thereby the water has a better opportunity of abſorbing the nitrous air. See p. 180 of this volume.

P. 254. It will be found that the diminution of this quantity of dephlogiſticated air did not proceed any farther in another whole year. See p. 154 of this volume.

P. 335. When I wrote the former part of this ſection, I concluded, and rightly, that the dephlogiſticated air was produced by the *green matter* in the water. But when I wrote the ſecond part, I imagined it to be produced by the influence of light upon the *water* itſelf diſpoſing it to depoſit the green matter. It will be ſeen in this volume that, having found this matter to be a plant, I preſently ſatisfied myſelf, that this, and all other plants, are capable, by means of the action of light upon them, to depurate the air to which they have acceſs, and thereby increaſe the quantity of it.

P. 458. It will be ſeen in this volume, p. 83, &c. that there was an addition of inflammable air from the iron filings and brimſtone in this proceſs.

## SECTION XXXII.

### *A ſummary View of all the moſt remarkable Facts in this and the four preceding Volumes.*

### PART I.

### *Facts relating to* COMMON *air.*

COMMON air is not affected by ſtagnation, i. 161, or by the cryſtallization of nitre, 161, 2, by the perſpiration of the body, iv. 275, v. 104, or by ſteam, iv. 281, v. 135.

Air extracted from pure water is generally purer than atmoſpherical air, v. 168, 170.

Many kinds of effluvium mix with it, but do not incorporate with it, i. 157, 2.

Common air is phlogiſticated and diminiſhed by charcoal, i. 129, 2, by the calcination of metals, 133, by paint, 138, by liver of ſulphur, 179, 2, by Homberg's pyrophorus, 179, 3, by the firing of gunpowder, 179, 4, by cement made with bees wax and turpentine 179, 5, by iron which has been expoſed to nitrous air, 181, 2, 222, by the electric ſpark, iv. 284, by nitrous ether, ii. 330, by the converſion of the blue calx of iron into red, iv. 289, by the ſolution of copper in volatile alkali becoming blue, 288, by water freſh diſtilled, 293, and by flowers, 311.

Fiſhes phlogiſticate the air combined with the water in which they live, iii. 341. v. 136. They die in water impregnated with phlogiſticated air, 138.

Common air is diminiſhed by candles burning in it one fifteenth, or one ſixteenth of its bulk. i. 44, It has by this proceſs received about one third of the phlogiſton that it is capable of receiving, 116, 2.

The diminution of common air by iron filings and brimſtone is between one fourth and one fifth of the whole, i. 105.

It may be repeatedly diminiſhed by nitrous air, and again cleanſed by agitation in water, till the whole would diſappear, i. 190, 2.

Common air is both phlogiſticated and abſorbed by oil of turpentine, iii. 94. Whenever it is phlogiſticated it is probable that part of it is abſorbed, 97. It is liable to be abſorbed by water, and the remainder is partially phlogiſticated, i. 158, 2.

Common air is improved by the growth of plants, iv. 300, 307. It is alſo improved by being incorporated with water, and kept there ſome time, 353.

## PART II.

### *Facts relating to* DEPHLOGISTICATED *air.*

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It is found in the bladders of ſea weed, iv. 313, in water, 354, 466, in ſea water, 356, 469.

It is produced by a green vegetable matter in water, iv. 338, but not without the influence of light, 342, 489, v. 18.

It is extracted by heat from red precipitate, ii. 35, from ſpirit of nitre and any kind of earth, 55. The ſame earth may be uſed repeatedly with freſh ſpirit of nitre, till it vaniſh, 56. It is produced in the greateſt abundance by the metallic earths, ii. 63, after theſe by the calcarious, ib. from minium by ſpirit of nitre, 53, from the earths of all the metals, iii. 6, from all other kinds of earth, 28. The quantity of it depends upon the quantity of ſpirit of nitre made uſe of in the proceſs, ii. 378.

Dephlogiſticated air may be extracted by heat from green vitriol, iv. 213, from the other metals diſſolved in vitriolic acid, 226, from blue vitriol, 227, from white vitriol, 228, from turbith mineral, 230, from earthy ſubſtances united with vitriolic acid, 236, from alum, 237, from quicklime and oil of vitriol, 238.

Dephlogiſticated air cannot be procured from any earthy matter diſſolved in ſpirit of ſalt, iv. 240; but it may be procured from ſpirit of ſalt impregnated with red minium, which would yield it of itſelf, 442; but not from the ſame acid impregnated from the ſame minium after the redneſs had been taken away by a previous *affuſion* of the ſame acid, ib.

An extremely pure kind of dephlogiſticated air is procured from mercury diſſolved in ſpirit of nitre, iv. 246.

Dephlogiſticated air is heavier than common air, ii. 94. It is purer than common air, or fitter for the combuſtion of inflammable ſubſtances, and for reſpiration, ii. 39, &c. 48, &c. It even ſerves for reſpiration longer than the degree of its purity, as indicated by the teſt of nitrous air, would lead us to ſuſpect, v. 156. Pyrophorus is fired in it, iv. 259. It is unfavourable to the growth of plants, iii. 336, iv. 326, v. 13.

When mercury is diſſolved in ſpirit of nitre, and dephlogiſticated air is afterwards extracted from it by means of heat, the whole of the mercury cannot be revived, iv. 260.

There is no acidity in this kind of air, nor in the reſiduum of red lead, out of which it was extracted, ii. 373, obſerved by Mr. Magellan.

Dephlogiſticated air contributes to the eaſy formation of precipitate per ſe, v. 152.

## PART III.

### *Facts relating to* PHLOGISTICATED *air.*

Phlogiſticated air is produced by charging common air with phlogiſton, i. 138, by the nitrous acid with animal ſubſtances, ii. 146.

It is lighter than common air, i. 46. It differs in this and other reſpects from fixed air, *Philoſophical Empiriciſm*, p. 42.

Phogiſticated air is reſtored by vegetation, i. 49, 87, iv. 299, 305, v 12. By this means proviſion is made for leſſening the effects of putrefaction in hot countries, the putrefactive matter in water ſerving for the nouriſhment of aquatic plants, v. 62. Phlogiſticated air is ſomewhat mended by agitation in yellow nitrous acid, iii. 128. It is not eaſy to convey it to any great diſtance in the ſame ſtate, iv. 270.

Several inſects will live very well in air tainted with putrefaction, though it is fatal to all animals that breathe it, i. 86.

## PART IV.

### *Facts relating to* FIXED *air.*

Fixed air is not yielded by pit coal, though the aſhes of it contain a great quantity of this air, iv. 393, but Bovey coal does contain fixed air, ib.

Fixed air is contained in ſaline ſubſtances, ii. 115, in vitriolated tartar and Glauber ſalts, v. 165, in alum, ib. It is extracted from the calces of metals by heat, ii. 111, and from clay, ii. 215. A great quantity of it, mixed with inflammable air, is contained in cream of tartar, iv. 403. It is retained obſtinately by cream of tartar expoſed to heat, iv. 405. It is extracted from the earth that Mr. Godfrey obtained from water, *Philoſophical Empiriciſm, Advertiſement.*

No vitriolic acid contained in fixed air obtained by the means of it, ſhewn by Mr. Hey, i. 288. Shewn by Mr. Bewly not to partake of the nature of the acid by which it is procured from calcarious ſubſtances, ii. 382. Proved by him to be a particular acid, 337.

A ſolution of mercury in ſpirit of nitre yields fixed air in conſequence of being expoſed to the atmoſphere, iii. 352, iv. 388. Wood aſhes imbibe fixed air from the atmoſphere, iii. 353, iv. 390; ſo alſo do pitcoal aſhes, iv. 392; but it is not attracted by bone aſhes, 394. It is procured from pitcoal aſhes repeatedly after being mixed with nitrous acid, iii. Preface 33. It is alſo generated repeatedly from wood aſhes and ſpirit of nitre, iii. 31, &c. and from minium with ſpirit of nitre, 35. Fixed air is procured from ſpirit of wine and ſpirit of nitre, 350; as alſo from vitriolic acid and ſpirit of wine, iv. 384, and from vitriolic acid and ether, 386.

The reſiduum of fixed air is the ſame thing with phlogiſticated common air, ii. 331. It has a reſiduum not imbibed by water after being expelled from water, 219.

A great quantity of fixed air is procured from mice putrefying in water, iii. 340.

It ſeems to be depoſited from common air by burning inflammable ſubſtances in it i. 44, but not when brimſtone is burned, 45.

45. It is found in common air reſtored by agitation in water, and then phlogiſticated by nitrous air, ii. 218, and in all caſes in which dephlogiſticated air is procured, even from precipitate per ſe 217. Leſs fixed air is diſcovered in common air when it is phlogiſticated by reſpiration, than by putrefaction v. 111, 118. None is found when it is phlogiſticated by the firing of inflammable air 124.

Fixed air contained in water is eaſily imparted to any kind of air that is tranſmitted through it, iii. 355. It is diſcharged from water by removing the preſſure of the atmoſphere i. 34. It is not imbibed by ice, 33. It does not of itſelf diſſolve iron, 215.

One meaſure of fixed air ſaturates almoſt three meaſures of alkaline air, iii. 293. It changes red roſe leaves white i. 36. iii. 316. It is fatal to vegetables i. 36. iii. 308, but not ſoon fatal to inſects, i. 36.

Water impregnated with fixed air is fatal to vegetables which have their leaves in it iii, 321. It kills the plants which have their roots in it, iv. 329. It kills fiſhes, ii. 231.

It prevents the putrefaction of fleſh meat, obſerved by Sir William Lee, iv. 461.

Fixed air becomes phlogiſticated air by the electric ſpark taken in it, i. 248, 2.

A ſaline ſubſtance is formed by fixed air with the earth of alum, iv. 445.

A clyſter of fixed air, adminiſtered by Mr. Hey, cures a putrid fever i, 292, Succeſsfully adminiſtered in a putrid diſeaſe by Dr. Warren, ii, 375. The efficacy of it in putrid diſeaſes obſerved by Dr. Dobſon, ii. 369 Various medicinal uſes of it by Dr. Percival i, 300. Water impregnated with it propoſed by Dr. Percival as a ſolvent for the ſtone in the bladder, ii, 360. A neutral ſalt compoſed by it recommended to the faculty by Mr. Bewly, 346, 398. An application of fixed air relieves an inflamed breaſt, obſerved by Mr. Adam Walker, iv, 464. Blood is not coagulated by it, obſerved by Dr. Falconer, i. 315.

## PART V.

### *Facts relating to* INFLAMMABLE *air.*

Inflammable air diſcovered in the bottom of ſtanding water, related by Dr. Franklin, i. 321.

It is procured from regulus of antimony in marine acid, iii. 255, from metals by the vegetable acid, 256, from cream of tartar, together with fixed air, iv. 403, more of it from ſteel than from iron, iii. 166, from copper by the marine acid, i. 144, from lead by the ſame acid, 145, from iron by heat, ii. 107, from iron filings and brimſtone in a warm place, iii. 258, and alſo in time in the common temperature of the atmoſphere, v. 83, from zinc and brimſtone in a warm place, iii. 259, from a ſolution of galls with iron filings, iv. 361.

Inflammable air is procured by taking the electric ſpark in oil, i. 244, in ſpirit of wine, 245, and in volatile ſal ammoniac, ib.

The electric ſpark taken in alkaline air produces three times the quantity of inflammable air, v. 218.

A conſiderable quantity of it was extracted from oil of turpentine, iv. 363. It was increaſed by agitation in oil of turpentine, iii. 266, and alſo in ſpirit of wine, but not in the ſame degree, ib. After this, its inflammability was much leſſened, ib.

A ſpecies of temporary inflammable air was made by Dr. Ingenhouſz from ether, iv. 474.

Inflammable air contains the ſame quantity of phlogiſton with nitrous air, bulk for bulk, iv. 378. It contains no acid, 364, 377. Water impregnated with it does not turn the juice of turnſole red, iii. 268.

Inflammable air recently made has a ſmell according to the ſubſtance from which it is extracted, i. 57.

It is not affected by the electric ſpark, iv. 367. The colour of an electric ſpark taken in it is red, i. 61. It is fired by Mr. Volta with an electric ſpark, iii. 382.

Inflammable air, by long ſtanding in water, is much diminiſhed in bulk, and becomes phlogiſticated air, i. 59. This proceſs is accelerated when the water has been boiled, 67, and by agitation, 68.

Inflammable air mixed with the fumes of ſpirit of nitre, is fired at one exploſion, i. 65. When it is agitated in pale ſpirit of nitre the quantity of it is increaſed, and it is fired with a ſtill greater exploſion, iii. 262. If the nitrous vapour continue long in it, the vapour is reimbibed by the acid, and the inflammable air is fired as uſual, 264.

Inflammable air mixed with fixed air burns with a blue flame, ii. 110. With nitrous air it burns with a green flame, i. 117.

When inflammable air is made reſpirable by agitation in water, and is then phlogiſticated by nitrous air, it does not make lime water turbid, i. 188.

Inflammable air is diminiſhed by florid blood, iii. 76. It is decompoſed by flint glaſs, in a red heat, making the glaſs black, iv. 368. The tranſparency of the glaſs is reſtored by heating minium in it, 376. Inflammable air is imbibed by water, and expelled again by heat, in the ſame ſtate, iii. 267. When imbibed by charcoal, it comes out leſs inflammable, but that which is not imbibed is as inflammable as ever, iv. 378. It is imbibed by the willow plant, 322.

Common air is phlogiſticated by decompoſing inflammable air in its naſcent ſtate, v. 92.

When animal ſubſtances putrefy, all the inflammable air they yield is extricated before the whole of the fixed air, iii. 343, v. 79.

Alimentary ſubſtances yield inflammable air by putrefaction, v. 65; but do not part with it in boiling, 74.

Inflammable air is fatal to animals, i. 62, benumbs waſps and other inſects, but does not kill them, 247.

Its refractive power is greater than that of common air, obſerved by Mr. Warltire, iii. 365.

## PART VI.

### *Facts relating to* NITROUS *air.*

Nitrous air is procured by diſſolving ſeveral metals in ſpirit of nitre, i. 110, from lead, ii. 174. Twice as much is got from mercury after

after it is completely diſſolved in ſpirit of nitre, than during the ſolution, iv. 266. Only one third of the quantity of dephlogiſticated air is procured from the ſame ſolution, ib. Some remarkable phænomena attend the ſolution of iron in nitrous acid with reſpect to the production of air, iii. 169.

The quantity of nitrous air does not depend upon the quantity of water in which the ſame metal is diſſolved, iii. 168. But the quantity of it is nearly in proportion to the quantity of water with which the acid is uſually diluted, in order to diſſolve any particular metal, ii. 225. But though water may poſſibly enter into the compoſition of it, v. 171, there is no water diſcovered in the decompoſition of it, 172. Almoſt three times as much is procured from iron, as from copper, iii. 166. It is procured from liquid ſubſtances containing phlogiſton, ii. 124, from gums, &c. ii. 125, from charcoal, 137. It is yielded by vegetable ſubſtances more than by animal ones, 142. Of animal ſubſtances it is moſt readily yielded by fat, and the brain, 157.

Nitrous air is produced by impregnating diſtilled water with nitrous vapour, iii. 199, and alſo with a pure vapour of nitrous acid, without the ſolution of any metal, iv. 66.

Nitrous air diminiſhes common air about one fifth, and itſelf wholly diſappears, i. 110. It diminiſhes no air but reſpirable air, 114.

Nitrous air is not changed by keeping two years in a phial cloſe corked, iii. 358. It is not changed by being expoſed to heat in a flint glaſs tube hermetically ſealed, iv. 46. Nor when confined with water in the ſame circumſtances, v. 177. Nor by expanſion with heat over quickſilver, and mixed with water, iv. 46.

Nitrous air is not heavier than common air, i. 119.

The nitrous acid that enters into the copper is ſix times as much as enters into the nitrous air produced by the ſolution of it, iii. 162.

Nitrous air is imbibed by water, and is expelled again by heat, without any change of property, iii. 109. It is alſo expelled from water by freezing, 359. The reſiduum that is not imbibed by water is phlogiſticated air, i. 120; but after much agitation in water it becomes reſpirable air, and is diminiſhed by freſh nitrous air, 189, and without forming any incruſtation when the proceſs is made in lime water, 190. Water impregnated with nitrous air depoſits a ſediment when it is frozen, iii. 559.

Water made blue with the juice of turnſole becomes red by being impregnated with nitrous air, iii. 108. In other reſpects it is not ſenſibly acid, till it be decompoſed by common air, as obſerved by Mr. Bewly, i. 318; but by the decompoſition of much nitrous air in contact with water a ſtrong nitrous acid may be procured, 320. Four ounces and a half of water will receive the nitrous acid from three hundred ounce meaſures of nitrous air, when it becomes blue, iii. 161. The acid in this water is extremely volatile, iii. 162.

An impregnation of nitrous air gives a purple colour to vitriolic acid, iii. 129, and a blue colour to ſpirit of ſalt, ib. It is alſo abſorbed by radical vinegar, and by water impregnated with vitriolic acid air, 130.

It gives a green colour to a blue ſolution of copper in ſpirit of nitre, v, 176.

Nitrous air agitated in nitrous acid is made, in a conſiderable degree, reſpirable, iii. 128.

When nitrous air is kept in a bladder, it never diminiſhes any kind of air without an appearance of fixed air, i, 191, 214. Water impregnated with it ſometimes makes a depoſit of white matter, iii. 104.

Nitrous air reſiſts putrefaction, i. 123. It preſerves animal ſubſtances, but not long in a ſtate fit for culinary purpoſes, iv. 69. Bile impregnated with it is long preſerved from putrefaction, 74.

Nitrous air is fatal to plants, i. 119. even to the willow plant, v. 13, and to inſects, i. 226.

Nitrous air becomes phlogiſticated air when diminiſhed by long keeping in water, iv. 62. When imbibed by charcoal, both the remainder, and that which is expelled from the charcoal by water, is phlogiſticated air, iv. 454. It is diminiſhed to one fourth by iron filings and brimſtone, i. 118, much more by liver of ſulphur, 219. When decompoſed

compoſed by iron filings and brimſtone, it imparts no acidity to the water in contact with it, iii. 143, 146.

Nitrous air is decompoſed by pyrophorus, iv. 64, by nitrous vapour, ii. 170. It is abſorbed in a very great quantity by the nitrous acid, iii. 122. It is diminſhed by florid blood 76. It is decompoſed by a ſolution of green vitriol, iii. Preface, 33. In this proceſs the ſolution becomes black, iv. 48, and this is the ſame whether the nitrous air has been got from iron or copper, 50. The ſolution recovers its colour by expoſure to the air, which it phlogiſticates, 52.

Nitrous air is decompoſed by olive oil, which is coagulated by it, iv. 75. It is readily abſorbed by oil of turpentine, which takes more than ten times its bulk of this air, iii. 112. It is abſorbed by ether, 115, by alkaline liquors, 118, by ſpirit of wine, 119.

Nitrous air is diminiſhed by being kept in a bladder alternately moiſt and dry, iii. 155. The water in contact with it in this proceſs is very acid, 157.

This air is diminiſhed very much by the electric ſpark, i. 222. iv. 64.

A great proportion of nitrous air becomes phlogiſticated air immiſcible in water by long keeping, v. 178.

### *Facts relating to* DEPHLOGISTICATED NITROUS *air.*

Nitrous air is partially dephlogiſticated by long expoſure to iron, i. 215, v. 194, 203. This dephlogiſticated nitrous air is procured immediately by the ſolution of tin, iii. 17, 24, by the ſolution of iron with heat, 133; but in this caſe it burns more like inflammable air, 134. It is alſo procured by the ſolution of zinc, 135. It is produced in great plenty by a ſolution of copper upon iron, v. 200. It is procured from iron filings and brimſtone by nitrous air before it becomes phlogiſticated air, iii. 141. It is procured ſuddenly after a conſiderable diminution of nitrous air, iv. 56. The ſlower the proceſs is, the more of the nitrous air will become phlogiſticated, 57.

Dephlogiſticated nitrous air is procured in great purity by making water imbibe it, and then expelling it again by heat, v. 213. It becomes purer air by being kept united to water, 214.

Alkaline air does not affect this air, v. 216; nor is the colour of juice of turnſole changed by it. When abſorbed by water, it imparts no acidity to it, iii. 143, 146.

## PART VII.

### *Facts relating to* MARINE ACID *air.*

Marine acid air is procured by heat from ſpirit of ſalt, i. 146, and from ſalt and oil of vitriol, 229.

It conſiſts of the marine acid in a ſtate of vapour, i. 147, and probably contains phlogiſton, ii. 5. It is heavier than common air, i. 241. It is converted into a white ſubſtance by heat, in a glaſs tube hermetically ſealed, iv. 101.

With alkaline air it forms the common ſal ammoniac, i. 170. One meaſure of this air abſorbs 1 1-6th of alkaline air, iii. 294.

Marine acid air extinguiſhes a candle with a blue flame, i. 147. It diſſolves iron, 149. It diſſolves ſulphur and nitre, 149. It coagulates oils, 150. It diſſolves ice, 140. It makes camphor fluid, 235. It makes blue vitriol green, 237. It diſſolves white vitriol, ib. It deprives borax of its water, ii. 238.

It makes inflammable air with many ſubſtances containing phlogiſton, i. 149, with wood, dry fleſh, &c. 231, with quick lime, 238. It alſo forms a permanent air with liver of ſulphur, ii. 233. It is a little diminiſhed by the electric ſpark, 239.

United with water it forms the marine acid, i. 148, and then appears to be twice as heavy as water, ib. It is imbibed by ether, 234.

## PART VIII.

### *Facts relating to* VITRIOLIC ACID *air.*

Vitriolic acid air is procured by heating in oil of vitriol almoſt any ſubſtance that contains phlogiſton, ii. 2, &c. but not from gold

gold or platina, 20. It extinguiſhes a candle without any particular colour, 7. It is heavier than common air, 7, and than alkaline air, 9.

Vitriolic acid air phlogiſticates common air, ii. 10. It will not diſlodge the nitrous acid, or the marine acid, from any ſolid ſubſtance in which they are contained, 11. It diſſolves camphor, 13. It deprives borax of its water, 14.

Sulphur is formed from water impregnated with vitriolic acid air in a long continued heat, iv. 124. White cryſtals are formed in a glaſs tube, containing vitriolic acid air expoſed to heat, 131.

Vitriolic acid air uniting with alkaline air makes vitriolic ammoniac ii. 9. A yellow ſubſtance is produced at the ſame time, 22; but it becomes white by expoſure to the common air, iii. 277. One meaſure of this air ſaturates two of alkaline air, 292.

Vitriolic acid air united with water makes volatile vitriolic acid, ii, 7. Water imbibes ten times more marine acid air than the vitriolic, iii. 275; but when fully impregnated with either

ther of them, it will not take any of the other 276. Water impregnated with vitriolic acid air in time diſſolves ſome metals, and yields inflammable air, 273. Alum is formed by earth of alum and water impregnated with this air, iv. 123. Water impregnated with this air freezes without loſing its air, iii, 361.

Whale oil imbibes ſix or eight times its bulk of this air, and becomes red, iii, 278. Olive oil imbibes the ſame quantity, and becomes firſt colourleſs, but afterwards of an orange colour, ib. Oil of turpentine alſo imbibes this air, and aſſumes an amber colour, ib.

The electric ſpark taken in vitriolic acid air, confined by quickſilver, produces a black ſubſtance, ii. 239. It is produced by exploſions, when much more electric matter than they conſiſt of will not do it, iii. 281. The vitriolic acid air is diminiſhed by this proceſs, 280, This black matter is the ſame whether the air was produced from copper, quickſilver, or any other ſubſtance, 282.

## PART IX.

### *Facts relating to* FLUOR ACID *air.*

Fluor acid air is procured by diſſolving the fluor in hot oil of vitriol, ii. 190. It is capable of being confined by quickſilver, 189.

Water ſaturated with this air gives out an air that has all the properties of vitriolic acid air, ii. 207. Water and alkaline air require the ſame quantity both of vitriolic acid air, and of fluor acid air, to ſaturate them, iii. 289. Something ſimilar to this air is procured by oil of vitriol and Mr. Canton's phoſphorus, ii. 212; but this may perhaps be ſulphur formed in the ſolution and ſublimed, iii. 287.

This air extinguiſhes a candle, ii. 197. It forms a white ſubſtance with alkaline air, ib. One meaſure of this air ſaturates two meaſures of alkaline air, iii. 292.

Very little of this air is abſorbed by quick lime, or chalk, ii. 200. It is abſorbed by charcoal, ruſt of iron, and alum, 200. It diſſolves nitre, 201. Borax becomes ſoft in it, 204. Fluor acid air confined in a glaſs tube, and heated, corrodes it very much, iv. 434.

Water admitted to fluor acid air becomes acid, and a white ſubſtance, called the fluor cruſt, is depoſited, ii. 190. Water impregnated with this air will not freeze, iii. 361, except with a great degree of cold, iv. 443.

This air is imbibed by ſpirit of wine, ii. 199, by vitriolic and nitrous ether, ib. and by oil of turpentine, 199, 211.

## PART X.

## *Facts relating to* ALKALINE *air.*

Alkaline air is produced by heat from cauſtic volatile alkali, i. 164, and alſo from ſal ammoniac and ſlaked lime, 166. It conſiſts of volatile alkali in the form of air, 164.

Alkaline air is heavier than inflammable air, i. 176; but lighter than marine acid air, ib. It does not unite with oils, 172. It takes water from alum, 174. It diſſolves ice, 176. It will not diſſolve copper, ii. 232.

The electric ſpark taken in alkaline air produces inflammable air, ii. 239, and the quantity of inflammable air is three times that of the alkaline air, v. 218, &c.

Uniting with fixed air, it makes the mild volatile alkali, i. 171; with marine acid air it makes the common ſal ammoniac, 170, 205, &c. with water it is the volatile ſpirit of ſal ammoniac, 167.

Of this air there was abſorbed

| | |
|---|---|
| By fluor acid air, | 1 19-20th oz. meaſures. |
| Vitriolic acid air, | 2 |
| Marine acid air, | 1 1-6th |
| Fixed air, - | 1 6-7ths, Vol. iii. 294. |

## PART XI.

### *Facts relating to the* NITROUS ACID.

The nitrous acid may be exhibited in the form of air for a ſhort time, without being ſo much loaded with phlogiſton as to form nitrous air, ii. 168. The vapour of this acid is colourleſs, 172. It is capable of being combined with phlogiſton without water, and then confined in glaſs veſſels, iii, 186. This nitrous acid vapour becomes of a deeper vapour by heat, 187. The phlogiſton leaves it to join with the air with which it is mixed, 192. Its redneſs diſappears when combined with a little water, 196. Water impregnated with nitrous vapour emits a great quantity of nitrous air, 198; and alſo when it is previouſly impregnated with vitriolic air, 222.

Water impregnated with nitrous vapour firſt becomes blue, and then green, when the production of nitrous air ceaſes; afterwards it is yellow, iii. 198. Water is increaſed in bulk one third by this impregnation, 202. The nitrous vapour with which water is impregnated is exceedingly volatile, 203, 204. Nitrous acid thus made contains more phlogiſton than the common ſort, 205. It makes no depoſit when mixed with a ſolution of ſilver in the nitrous acid, ib.

Nitrous vapour is imbibed by animal oils, and emitted again by heat in the form of phlogiſticated

phlogiſticated air, iii. 182. Oils impregnated with nitrous vapour become red, but they are blue while hot, in the courſe of the proceſs, 208. Nitrous ether is made by impregnating ſpirit of wine with nitrous vapour, 213. The nitrous vapour ſeizes upon the water of alum, 229.

Nitrous vapour is imbibed by red lead, which then becomes white, iii. 230. The effect is the ſame whether the vapour has been produced by the ſolution of biſmuth, or of iron, iv. 36. All metallic calces have a ſtrong affinity with the nitrous acid, and when united with it become white, v. 236, &c. Theſe nitrated calces may be procured by diſtilling the ſolutions of theſe metals, 238. When the acid is expelled from them by heat, they will attract it again, 241.

Solutions of copper and mercury in nitrous acid, in a confined and long continued heat, from ſaline ſubſtances that are not deliqueſcent, iv. 489. A ſolution of iron is much ſooner converted into ſuch a ſalt, v. 234.

Nitrous vapour is imbibed by oil of vitriol, and ſeparated from it by water, iii. 218. Oil

of vitriol ſaturated with it is cryſtallized with it, iv. 26, 450. All the liquid in the compoſition is then pure ſpirit of nitre, 33. If phlogiſton be contained in the oil of vitriol, it paſſes into this nitrous acid, 452.

Nitrous vapour converts ſpirit of ſalt into the beſt aqua regia, iii. 219. It makes various changes in ſeveral fluid ſubſtances, iv. 38.

Spirit of nitre may be procured almoſt colourleſs by a careful diſtillation in the common way, iv. 453. From being of a deep orange, it becomes green by long keeping, 453; afterwards of a deep blue, 454. But if it be expoſed to the open air, it becomes orange coloured again, ib.

Nitrous acid phlogiſticates common air, ii. 163, 167. It has the ſame effect when it is quite colourleſs, iv. 25. Heat deepens the colour of this acid, iii. 249. Its colour is univerſally owing either to phlogiſton, or heat, iv. 2.

The nitrous acid diſſolves aſtringent vegetables with great rapidity, iii, 170. The pale acid, in diſſolving copper, yields leſs nitrous

trous air at firſt, and moſt afterwards; whereas the ſmoaking ſpirit yields the moſt air at firſt, and makes a hiſſing noiſe when mixed with water, 245. This acid gives more nitrous air by the ſolution of metals, when it has been volatilized, by being heated together with earthy ſubſtances, than it does by the ſolution of the ſame metals without being volatilized, 44, 50, 51, 357. The ſame effect is produced when the acid is volatilized by the ſolution of biſmuth, and then imbibed by water, 250, 356.

Nitrous acid is not eaſily combined with water after being volatilized, iii. 48, 253. It is phlogiſticated and volatilized by nitrous air, 126, 441.

A white ſubſtance is produced when the nitrous acid is heated a long time in glaſs tubes hermetically ſealed, iv. 21. When this acid is mixed with the vitriolic, and iron is diſſolved by them, the firſt produce is nitrous air, and the ſecond inflammable, iii. 171.

## PART XII.

## *Facts relating to the* MARINE ACID.

Marine acid has the ſame affinity with earthy ſubſtances, whether combined with water, or in the form of air, iv. 459.

The colour of marine acid is owing to earthy impregnations, iv. 79. A different ſubſtance generally gives it a different colour, 86. The colour of it is diſcharged by the coal of cream of tartar, 109, by liver of ſulphur, 111, and by flowers of zinc, 112. It recovers its colour by expoſure to the air, if it has been diſcharged by liver of ſulphur, 112, but not if it has been diſcharged by flowers of zinc, 458.

The marine acid ſaturated with the ruſt of iron depoſits ſomething when it is hot, iv. 103, as alſo do other ſaturated ſolutions of this acid, ib. An incruſtation is made in glaſs tubes hermetically ſealed, containing a ſaturated ſolution of common ſalt, expoſed to a continued heat, 106.

The marine acid when hot diſſolves glaſs, iv. 93.

The marine acid is dephlogiſticated by the calces of lead and manganeſe, ſo as to be incapable of yielding any marine acid air that can be confined by mercury, v. 252.

## PART XIII.

## *Miſcellaneous Facts relating to* ACIDS.

Vitriolic acid is coagulated by quicklime, ii. 229. It depoſits an earthy matter at the firſt diſtillation, but not afterwards, iv. 116.

A mixture of the vitriolic and nitrous acids, though ſeparately coloured, becomes colourleſs, and a white depoſit is always made, iv. 439. The nitrous acid entirely eſcapes from this mixture, v. 243, but more readily when it is expoſed to nitrous air, 246.

The phoſphoric acid is not convertible into air, iv. 135, not even with ſubſtances containing phlogiſton, ib. It yields inflammable air with minium, 136.

## PART XIV.

## *Miſcellaneous facts relating to* AIR.

No air is produced from radical vinegar, even with any ſubſtance containing phlogiſton, iii. 329; or from ſmoking ſpirit of Libavius, 330; none from cauſtic alkali and any metal, 332; none from ſpirit of wine, or ſpirit of wine and camphor, ib. The principle of ſmell does not ſeem to be capable of being exhibited in the form of air, ib. No air is procured from concentrated nitrous acid and copper, iv. 441.

More inflammable air is procured from vegetable and animal ſubſtances by a quick than by a ſlow proceſs, i. 56, iii. 255. More dephlogiſticated air is alſo produced in the ſame manner, 337.

Different kinds of air that have no affinity do not, when mixed together, ſeparate ſpontaneouſly, but continue diffuſed through each other, iii. 301, iv. 432.

The different kinds of air are expanded by the addition of ten degrees of heat according to Fahrenheit's thermometer, in the following proportion.

| | |
|---|---|
| Common air - - | 1.32 |
| Inflammable air - | 2.05 |
| Nitrous air - - | 2.02 |
| Fixed air - - | 2.20 |
| Marine acid air - | 1.33 |
| Dephlogiſticated air - | 2.21 |
| Phlogiſticated air - | 1.65 |
| Vitriolic acid air - | 2.37 |
| Fluor acid air - | 2.83 |
| Alkaline air - - | 4.75 |

Urine phlogiſticates dephlogiſticated air, and decompoſes inflammable and nitrous air, v. 132.

## PART XV.

### *Facts relating to* MERCURY.

Mercury containing lead, &c. is cleanſed by agitation in reſpirable air, which it phlogiſticates,

giſticates, iv, 149. It is converted into a black powder by agitation in water, 159; and in ſpirit of wine, 161. This black powder is mercury ſuper-phlogiſticated, and it becomes running mercury by expoſure to the air, which it phlogiſticates, 162, &c. When the water is warmed, it imbibes the phlogiſton which made the mercury black, 169. Mercury may be exhibited in four different ſtates in ſucceſſion, beginning with running mercury, and ending with the precipitate per ſe, by expoſure to heat on a glaſs plate, 175.

Water that has been often uſed for agitating mercury has a greater effect in that proceſs than water that has not been uſed for this purpoſe, iv. 176.

Mercury is preſently divided into ſmall globules by agitation in vinegar, iv. 181. The vapour of mercury in the temperature of the atmoſphere eaſily pervades vitriolic acid air, v. 225, &c. It is ſuper phlogiſticated by the electric ſpark taken in it, ib.

Precipitate per ſe is formed by long agitation of mercury in a glaſs veſſel cloſe ſtopped, iv. 186. The production of it is favoured by dephlogiſticated air, v. 152.

## PART XVI.

## *Facts relating to* ELECTRICITY.

The conducting power of charcoal is greater in proportion to the heat with which it is made, ii. 245, &c. A ſubſtance which has a peculiar conducting power is formed from turpentine, and other vegetable oils, burned with glaſs in a confined place, 257.

The electric exploſion diminiſhes common air more than the ſame quantity of electric matter in ſparks; the glaſs tube in which it is taken is covered with a black ſubſtance, which is from the mercury, by which the air was confined, 3. Preface, 34.

The electric matter leaves an interrupted circuit and paſſes into any neighbouring conductor, but leaves it the ſame inſtant, v. 258, &c.

The electric exploſion may be taken in the hot vapour of water, and of quick-ſilver, iv. 426.

An exploſion may be tranſmitted through glaſs much ſhort of a red heat without breaking it, v, 293.

A thick glaſs tube burſt in a remarkable manner by an electric exploſion, iv. 428. A coating of cement favours the burſting of glaſs jars by electric exploſions, v. 287, &c.

## PART XVII.

### *Facts relating to a* LONG CONTINUED HEAT.

In a long continued heat iron is depoſited from a ſolution of it in water impregnated with fixed air, iv. 413. Mercury and copper are alſo depoſited from a ſolution of them in ſpirit of nitre, 414. The ſaline ſubſtances thus formed are not deliqueſcent, 489. Gold is partly cryſtalized, and partly depoſited, from a ſolution in aqua regia, 416.

An incruſtation is formed in a glaſs tube containing a ſolution of nitre, iv. 415. Lime is depoſited from lime water, 413. A white ſubſtance is depoſited from cauſtic volatile alkali, 422. The colour of olive oil is changed by expoſure to heat, 419.

## PART XVIII.

## *Facts relating to* MINERAL SUBSTANCES.

Brimſtone affects copper duſt in the ſame manner as it does iron filings, i. 157.

Minium contains no nitrous acid, iii. 45. The colour of minium is the ſame with that of blood. It alſo becomes darker by heat, and recovers its light colour by expoſure to the air, iv. 429. Minium is capable of yielding pure air ſo long as it retains its red colour, and no longer; but when deprived of it by ſpirit of ſalt it ſtill retains that property, 432.

Biſmuth and nickel diſſolved in marine acid give a ſmell of liver of ſulphur, i. 161.

The ſolution of copper in volatile alkali becomes blue by phlogiſticating the air contiguous to it, iv. 288. The calx of iron, from being blue, becomes red by parting with its phlogiſton to the air, 289. Water takes phlogiſton both from phlogiſticated and from inflammable air agitated in it, i. 68. The vapour of water much heated corrodes both glaſs and iron, iv. 410.

## PART XIX.

## *Facts relating to the* VEGETABLE SYSTEM.

Growing vegetables purify air plogiſticated by the burning of candles, i. 49, by reſpiration or putrefaction, &c. 87. iv. 305. They imbibe what would contribute to putrefaction, and yield an offenſive ſmell, v. 43, 57.

The willow plant abſorbs air of various kinds, iv. 320; and thrives beſt in inflammable air, v. 1.

Light is neceſſary to enable plants to purify air, v. 18. But pure air is not *produced* by light or by plants, but only by the purification of the impure air to which the plants have acceſs, 24, 27, 29.

Air is ſtrained through the body of a plant, v. 11. Pure air is found in the bladders of ſea weed, iv. 313. Green vegetable matter or *conferva minima* produces pure air in the light, iv. 337, &c. 489. Heat has no ſuch effect, 346, &c.

The ſeeds of this plant float in the air, v. 34. It does not appear ſoon in rain water, or diſtilled water, 35, 36. It does not appear in water till the fixed air it contained be expelled from it, 39.

Potatoes are favourable to the production of this vegetable ſubſtance, v. 49. Onions are unfavourable to it, v. 52, ſo are blood, fat, gall and gravy, 61, 10, ſo alſo are fruits, 73.

## PART XX.

### *Facts relating to the* ANIMAL OECONOMY.

Air is in different ſtates at different times in the bladders of fiſhes, ii. 230.

Blood has an attraction for phlogiſton, and when ſaturated with it is of a darker colour, iii. 56, 71. It attracts phlogiſton through ſerum and a bladder, iii. 78.

Animal ſubſtances, in putrefying, yield both fixed air and inflammable air, but the inflammable air is exhauſted firſt, iii. 343; v. 79.

The perſpiration of the body does not injure air, iv. 275, v. 104.

No air iſſues from the pores of the ſkin, v. 100.

Animals die in noxious air ſuddenly, 71.

Gall yields a great quantity of nitrous air when diſſolved in the nitrous acid, iii. Preface 32.

Animal ſubſtances are not reduced in their dimenſions by coaling, ii. 244.

## PART XXI.

### *Miſcellaneous facts.*

Charcoal is expanded by heat, like metals, ii. 256.

Lime water does not freeze ſo ſoon as common water, iv. 444. Lime water impregnated with vitriolic acid air does not freeze ſoon, ib.

Homberg's pyrophorus proved by Mr. Bewly not to owe its accenſion to vitriolic acid, but probably to an affinity between that acid which is the conſtituent part of the atmoſphere and the alkali in that compoſition, iii. 395. A purely alkaline pyrophorus diſcovered by him, iv. 479.

Bolognian phoſphorus ſhewn by profeſſor Allamand to give out light of the very ſame colour with that which it had imbibed, iii. 376.

Sound originates in different kinds of air, and its intenſity is nearly in proportion to their denſity, v. 295.

A volatile alkali is found in the ruſt of iron produced by the ſolution of copper in the nitrous acid, v. 301.

## SECTION XXXIII.

### *Experiments and obſervations made after the preceding ſections were ſent to the preſs,*

### § 1. *Of the reſpiration of dephlogiſticated air.*

I HAVE obſerved, p. 163, that I could never make mice live in dephlogiſticated air till they had completely phlogiſticated it, and I could not, at the time of writing that article, aſſign any ſufficient reaſon for the fact. Being unwilling to leave this difficulty unſolved, I repeated the experiment; and putting a vigorous mouſe into about ten ounce meaſures of this air, it continued ſome hours ſeemingly at its eaſe, but died while the air was ſo pure, that, with two equal quantities of nitrous air, the meaſures of the teſt were conſiderably leſs than 1.0.

I then put another young mouſe into the remainder of the air, and it alſo continued at its eaſe two or three hours; but then ſeemed to be expiring, its reſpiration being very languid, and ſo ſlow, that I ſeveral times concluded it was abſolutely dead. I did not at firſt ſuſpect that it could be affected by *cold*, when other mice lived very well in a wire cage in the ſame room; for it had ſoon become quite dry after paſſing through the water, and had never ſhewn any ſign of uneaſineſs. But bringing it near the fire, when the heat was about 80 or 90 degrees (though I removed it occaſionally when it ſeemed to be uneaſy on that account) it lived ſeveral hours longer, and when it died the air was as completely phlogiſticated as commonair is generally found to be when mice have died in it.

This experiment fully ſatisfied me, that it was nothing in the dephlogiſticated air itſelf that was the reaſon that mice could not live in it. I obſerved before, vol. i. p. 9, that a mouſe is a tender animal, and after paſſing through water, requires a conſiderable degree of warmth; but I did not ſuſpect that it required ſo much, and ſuch a continuance of it, as I found in this particular caſe.

## § 2. *Of the quantity of dephlogiſticated air that may be procured from nitre.*

The quantity of dephlogiſticated air that is yielded by nitre is ſtated by the Abbé Fontana at a hundred ounce meaſures from an ounce. I alſo found the ſame produce when I uſed a coated glaſs retort, and ſuch a heat as that veſſel would bear. But from two ounces of nitre, in an earthen retort, which Mr. Wedgwood was ſo obliging as to make for me of a peculiarly refractory earth, and in an intenſe white heat, raiſed by ſuch a furnace as Dr. Black has conſtructed, I got five hundred ounce meaſures of air, the whole conſiderably dephlogiſticated, and with very little fixed air. The firſt produce was ſo pure, that with two equal quantities of nitrous air, the meaſures of the teſt were 0.7; but the laſt part of the produce came ſlowly, and the meaſures were 1.3, which ſhews that the air was conſiderably injured by ſomething in the retort. For the air procured by this proceſs in glaſs is very pure, the meaſures of the teſt being generally leſs than 0.5.

What remained in the retort was a dark green or blue ſubſtance, extremely acrid to taſte, and deliqueſcent, weighing, when it was taken out of the retort, about 18 dwts. The air would have weighed about 13 dwts. and the water of cryſtallization which is 18 parts in 110 of the water, 6 dwts. and a half, ſo that three dwts. remain to be accounted for of the two ounces. This laſt was, in part, an *acid vapour* diffuſed through the air, and not incorporated with it; for whenever I emptied any of the veſſels of this air I perceived a very pungent ſmell. Part of the loſs alſo was the *white cloud* with which the air was often filled when it was produced.

## § 3. *Of dephlogiſticated nitrous air.*

That this ſpecies of air is really a dephlogiſticated nitrous air, and not a phlogiſticated air, as I had originally ſuppoſed is more evident from its being produced from nitrous air by the *ſcales of iron*, which fly off when it is hammered, and which are iron partly reduced to a calx. I filled a phial with theſe ſcales, and then filling it up with mercury, diſlodged the mercury with nitrous air. In this ſtate the phial continued near three weeks; when I found the air diminiſhed, I did not note how much, but a candle burned in the remainder juſt as if the nitrous air had been expoſed to iron. Theſe ſcales wanting much phlogiſton to make them iron, muſt be in a ſtate more ready to receive than to give phlogiſton to nitrous air.

Since I wrote the ſection relating to this ſpecies of air, I have produced it much more readily than in the method there deſcribed, viz. by applying *heat* to the veſſel in which it is produced. But I ſhall firſt obſerve that, immediately after producing a quantity of this air, in the manner directed, p. 200, I filled the veſſel up with water inſtead of the ſolution of copper; and, in about two days, the veſſed (which was a phial, containing near a quart) produced about three ounce meaſures of air, and was phlogiſticated air, extinguiſhing a candle.

Then, pouring out the water, I filled it again with the ſolution of copper, and put it into a pan of water, which I heated till it boiled; when, by means of a cork and bent glaſs tube, &c. I received from it about a quart of air, the firſt part of which was phlogiſticated (owing perhaps to its having been in a ſtate of yielding that air immediately before this proceſs) but afterwards it was a proper dephlogiſticated nitrous air, admitting a candle to burn in it quite naturally.

In order to aſcertain more exactly the degree of purity to which I could bring this air, I impregnated a quantity of ſnow water with it, and expelling it again by heat, I found that only one ſixth of what had been expelled from the water would not be imbibed by water again; ſo that, in this method, it may be procured in a ſtate of conſiderable purity.

Having a quantity of air in this ſtate, and having found that a candle burned very well in it, I put a mouſe into it; but it would have died very ſoon if I had not withdrawn it. This was on the 17th of March 1781. But on the 21ſt of the ſame month, I put another mouſe into the very ſame air, and was ſurprized to find that it continued perfectly at its eaſe five minutes. To be quite ſure with reſpect to the other properties of this air, I withdrew the mouſe while it was quite vigorous, and found that a candle burned very well in the air, but it was not in the leaſt affected by nitrous air. In this very ſingular caſe, nitrous air fails to be a teſt of the reſpirability of air. The air made uſe of in this experiment had been kept in a cup of mercury, but ſome water was in the veſſel along with it, and this water had imbibed a little of the air.

I then expelled more of this air, from ſome water that had been impregnated with it on the 17th day of March, but a mouſe died it, and almoſt as ſoon as it would have done in any other kind of noxious air. Theſe facts afford more matter for ſpeculation; but I ſhall make a few more experiments before I give myſelf much ſcope in that way.

## § 4. *Of a ſolution of copper in volatile alkali expoſed to heat.*

I have obſerved that a non deliqueſcent ſaline ſubſtance is depoſited from ſolutions of copper, mercury, and iron in the nitrous acid, expoſed to a continued heat. I have likewiſe found a ſimilar reſult with reſpect to a ſolution of copper in volatile alkali. In one day a ſimilar depoſit was made from this ſolution in the ſame circumſtances. The ſubſtance depoſited was of a dark blue colour. and adhered firmly to the glaſs; and when the veſſel was firſt opened, there was a pretty ſtrong ſmell of volatile alkali.

## § 5. *Of the power of the different kinds of air to conduct heat.*

One of the firſt things that I propoſed to try, with reſpect to the different kinds of air, was to obſerve their *power of conducting heat*, and I had a contrivance for that purpoſe when I was at Leeds. The thing, however, appearing to me to be of leſs conſequence than other things that I had in view I deferred it till very lately; when, in conſequence of the doctrine of heat becoming, by means of Dr. Crawford's book, the ſubject of general converſation, I determined to execute what I had ſo long projected.

For this purpoſe I prepared the veſſel repreſented fig. 2. and deſcribed in the *Introduction*. The thermometer was a very ſenſible one, and the ſcale large, ſo that I could mark upon it twenty diviſions, each larger than half an inch between the mean temperature of the atmoſphere, and a heat much below that of boiling water. After ſeveral trials I at length adjuſted it in ſuch a manner, that, having filled the veſſel with any kind of of air, I could plunge it to a certain depth, firſt in *hot*, and then in *cold* water, ſo that the mercury would riſe to the diviſion 20, and fall to that of 6 or 7 in a reaſonable time. I had a clock that beat ſeconds cloſe by me, and was ſo ſituated, that I could not well make a miſtake of more than two ſeconds, in noting the time when the mercury came to any particular diviſion. The precautions I uſed to plunge the veſſel the *ſame depth* in the water, in all the experiments, and to exclude all other differences, except what might be occaſioned by the different kinds of air, it would be tedious to recount; and no perſon converſant in experiments, and who is diſpoſed to repeat them, will need to be ſo minutely inſtructed.

I will juſt obſerve, however, that in the veſſel I uſed for *hot* water, I always made it boil, and it was ſo full, that the plunging of my air veſſel into it made it run over; and the veſſel for cold water was always freſh brought from the ſame pump. The mouth of the air veſſel was in a cup of mercury, always filled to the ſame height; and by this means I could try in the ſame manner even thoſe kinds of air, that could not be confined by water.

The beſt account that I could give of the reſult of theſe experiments would be to exhibit them in the form of *tables*, of the time at which the mercury reached all the degrees of the ſcale, both in aſcending and deſcending; which tables I have drawn up. But this I ſhall defer till I have an opportunity of repeating all the obſervations. At preſent I would only obſerve, that all the differences were not ſo ſtriking as I expected to have found them, but that *inflammable air* conducted heat much better than any other kind of air, the mercury aſcending the ſame ſpace in about half the time that it took up in common air. *Fixed air*, and all the kinds of *acid air* conducted heat conſiderably worſe than common air. *Alkaline air* conducted heat rather better than the acid airs, and dephlogiſticated air a little worſe than common air, but ſo little, that I would not anſwer for the ſame reſult in repeating the experiment.

N. B. In the courſe of theſe experiments, I could not avoid obſerving ſo great an expanſion of *alkaline* air by heat, that I conclude the obſervation in vol. iii. p. 347, may be perfectly accurate, though the extraordinary nature of it made me entertain the doubt I have there expreſſed concerning it.

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# APPENDIX.

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## NUMBER I.

*Extract of a Letter from* MR. ARDEN, *Lecturer in Natural Philoſophy, dated September* 25, 1772.

ABOUT fourteen or fifteen years ago, in the preſence of William Conſtable, Eſq; at his ſeat at Burton Conſtable, in Holderneſs, I made the following experiments.

I placed a large coated jar that would hold three or four gallons, directly under the prime conductor of a very good electrical machine. The prime conductor was at leaſt eight, or ten inches above the top of the jar, and the communication was made by a braſs wire bent at one end over the prime conductor, and the other end paſſed through a ſmall glaſs tube (contrived by Mr. Conſtable to prevent the electric matter from eaſily flying off) was ſuſpended in the middle of the jar, and had a ſmall piece of braſs chain faſtened to it, that reſted on the bottom of the jar.

I then began to turn the wheel, and after turning about 100, or 150 times, as low in the jar as I could ſee for the coating, I perceived a ball of fire, much reſembling a red hot iron bullet, and full three quarters of an inch in diameter, turning round upon its axis, and aſcending up the glaſs tube that contained the braſs wire, which was the conductor to the inſide of the jar.

I immediately aſked Mr. Conſtable if he ſaw the ball of fire? He ſaid, Certainly. I ſaid, I will turn on. He anſwered, By all means. I kept turning the wheel, and the ball of fire continued turning upon its axis, and aſcending up the glaſs tube till it got quite upon the top of the prime conductor. There it turned upon its axis ſome little time, and then gradually deſcended, turning upon its axis as it had done in its aſcent, and ſo continued till it was ſo much below the top of the coating that we could no longer ſee it. But ſoon after this, a very great flaſh was ſeen; a large exploſion was heard, and ſtrong ſmell of ſulphur was perceived all over the room, a round aperture was cut through the ſide of the jar, as fine as if it had been cut with a diamond, rather more than three quarters of an inch in diameter, and between two and three inches below the top of the coating, and the coating was torn off all round the aperture, about three or four inches in diameter. The jar was a pretty ſtrong one, of crown glaſs.

I then took another jar, ſo like the firſt, that when both were whole I could not eaſily perceive any difference between them. I then attempted to charge this jar, in the ſame manner as the other, and we both obſerved it very accurately. No ball of fire was ſeen, but preſently the jar diſcharged itſelf with a great flaſh, and exploſion, and at about the ſame part as of the firſt jar, but inſtead of the aperture which was made in the firſt jar, there was a circle about three quarters of an inch diameter, as white as chalk, and the coating torn off round about it as before. Upon touching the white part, it dropped out; and appeared to be glaſs in a fine powder.

We broke ſeveral other different ſized jars that day (which made Mr. Conſtable ſay we were in great luck) but without any thing elſe remarkable.

The firſt experiment was made ſoon in the afternoon of a clear day, and the machine ſtood directly between us and a window, which was not above a yard from it. I don't hear that this ball of fire has been produced by art by any one elſe to this day, although it is often ſeen produced by nature.

I had the pleaſure of ſeeing Mr. Conſtable this day, and of reading the account of theſe experiments to him, and to the beſt of his memory, he thought the whole was ſtrictly true.

Mr. Conſtable thinks it would not be difficult to repeat the experiment, and to produce the ball of fire at any time, provided the jar is large, and not coated too near the top, and that the wire communicating from the prime conductor, to the inſide of the jar, is made to paſs through a ſmall glaſs tube (which is certainly of great advantage in making experiments of this kind) and that the machine acts very ſtrong. If not, it will be in vain to attempt it.

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The fact mentioned in the preceding letter is of a very remarkable nature, and being perfectly well aſcertained, it is of importance that it be generally known, and kept in view. For though no perſon that has hitherto been made acquainted with it has been able to repeat the experiment, others may be more fortunate. Dr. Franklin, and, if I miſtake not, Mr. Canton alſo, and myſelf, were preſent when Mr. Henley endeavoured to produce this appearance; but though every expedient that any of us could ſuggeſt was made uſe of, we had no ſucceſs, and I have ſeveral times attempted it in vain ſince. I ſhall not, however, deſiſt from my attempts.

Mr. Arden's own evidence is abundantly ſufficient to authenticate the fact, and I have ſince had from Mr. Conſtable himſelf the ſame account of it. Could we repeat this experiment, there would not, I think, be any natural phænomenon in which the electric fluid is concerned, that we could not imitate at pleaſure. This circumſtance alone makes it a very intereſting object of inveſtigation.

## NUMBER II.

### *Extract of a Letter from* MR. BEWLY, *containing Obſervations on ſome Parts of this Volume.*

PAGE 115, &c. The following obſervations on this ſubject may perhaps deſerve your attention. I had long ago obſerved, that on breathing through an infuſion of litmus, the ſame change of colour was produced as when it was expoſed to the action of fixed air, or other acids. I put about two ounces of the infuſion into two tall cylindrical glaſſes; one of which ſtood on the table as a ſtandard, while I breathed through the other. Before the end of the third expiration the latter became red. I then added to it two drops of a ſaturated ſolution of fixed alkali; which reſtored to the infuſion it's blue colour. After three deep expirations, however, the liquor became again red. I then added to it ten drops more of the *lixivium tartari*; and after about 35 or 40 expirations, the liquor was again changed red; or, to uſe the language of my former letters on fixed air, the alkaline liquor was not only neutralized, but ſuper-ſaturated, or acidulated, with the *mephitic acid*, as I ſuppoſe this to be: the red colour, thus given to the infuſion, flying off when the liquor was expoſed to the air; in the ſame manner as when it has been impregnated with fixed air.

Theſe trials ſeem to prove that the quantity of fixed air, or at leaſt of a certain volatile acid, emitted in breathing is not inconſiderable: as in three expirations enough was detained in the liquor to more than neutraliſe two drops of *lixivium tartari*. By a different mode of experimenting (in cloſe veſſels) perhaps the quantity of fixed air precipitated from the common air inſpired into the lungs might be aſcertained with ſome degree of accuracy.

The reſult of the following experiment was unexpected. I breathed through two ounces of pure water ten or twelve times; expecting that, on immediately pouring into it a ſmall quantity of a ſtrong infuſion of litmus, the liquor would become red: but no change of colour was obſerved; and three expirations were requiſite, as before, to produce a red colour. This ſeems to ſhew that water alone will not ſeparate fixed air from the atmoſpherical air that has been expired from the lungs; but that the litmus performs that office. An accident has prevented me from proſecuting theſe experiments; and particularly from trying whether an alkaline ſalt diſſolved in *pure* water would be neutraliſed by the air expired from the lungs.

P. 218, &c. Is it neceſſary to ſuppoſe that the electric matter itſelf furniſhes the phlogiſton to the inflammable air, into which alkaline air appears to be converted? Volatile alkali in itſelf contains a very large portion of inflammable ſubſtance. Thus nitrous ammoniac, or *nitrum flammans*, that is, volatile alkali neutraliſed with ſpirit of nitre, deflagrates without any addition of inflammable matter, or by means of heat alone. The electric fluid may poſſibly add to the quantity of inflammable matter contained in the alkaline air, by carrying phlogiſton with it from the bodies that *conduct* it: but I think it's principal and moſt ſtriking effect, in this curious experiment, is it's withdrawing the volatile alkali from our cogniſance; poſſibly by cauſing it to form a combination with ſome other principle, which renders it inſoluble in water.

But may not the electric ſpark, in this caſe, act merely by it's *heat*; and might not dry alkaline air, confined in a tube made red hot, undergo ſome ſuch change? I do not remember that you ever tried *dry* alkaline air, in your experiments with tubes in a ſand heat; but in your fourth volume, cauſtic *ſpirit of* ſal ammoniac (as it ought to have been printed, page 422, line 7 from the bottom) preſented an increaſe of elaſtic matter; though it had been expoſed to little more than a boiling heat.

P. 225. I long ago obſerved, with no ſmall degree of ſurpriſe, the evaporation and condenſation of mercury at the top of a barometer in my poſſeſſion, which terminates in a ball; nor can I now account for ſome of the ſingular phænomena preſented by it. It is placed on the ſouth ſide of a window facing the weſt; and at the diſtance of nine feet from the fire. On inclining the tube, ſo as to fill the bulb, and then erecting it; I could, within twenty-four hours, with a ſtrong magnifier, perceive ſome hundreds of globules condenſed on that ſide of the bulb which faced the window or the light. Theſe globules increaſed daily in number and ſize; ſo as, in a few days, to be viſible with the naked eye; and at laſt to fall down, and be ſucceeded by others. Theſe appearances have preſented themſelves at all times of the year; nor does a ſingle globule appear on any other part of the bulb.

But the moſt ſingular circumſtance is that, on turning the tube half round, freſh globules would appear on the ſide now facing the window; while thoſe now turned to the oppoſite ſide would gradually diminiſh in bulk and number, and at length totally diſappear. This diſappearance cannot reaſonably be aſcribed to the ſuperior heat of that ſide of the bulb which now faced the fire place: as the ſame events have conſtantly taken place, when there was no fire in the room. I cannot plauſibly account for theſe appearances, by the mere influence of heat and cold, on the oppoſite ſides of a bulb not one inch and a quarter in diameter; in which the ſide facing the window, on which the globules were *condenſed*, in the *hot months*, muſt neceſſarily be much warmer than the oppoſite ſide facing the fire, in the *winter*; from which nevertheleſs the globules would evaporate and diſappear.

P. 234, &c. In a late publication, M. de Waſſerberg (*Inſtitut. Chem.* Tom. iii) aſcribes a ſomewhat ſimilar property to the nitrous acid when combined with *biſmuth*. He affirms that, whatever methods he uſed to produce a perfectly ſaturated ſolution of this ſemi-metal in ſpirit of nitre, he could not ſucceed. On adding more biſmuth to a ſolution of this kind, a copious precipitate falls to the bottom; and the ſolution ſtill continues ſtrongly acid.

## NUMBER III.

### *Obſervations on this Volume, with which I was favoured by* MR. WATT.

PAGE 149. I ſuppoſe the white powder to be lead, ſublimed by the help of the acid, and which would not be diſſolved by ſpirit of ſalt, which forms with lead an inſoluble ſalt, called *plumbum corneum.* The orange colour was from ſome phlogiſtic matter the lead had attracted.

P. 226. Vitriolic acid air, being a volatile ſulphur, ought to diſſolve and volatilize mercury, and would, on receiving more phlogiſton, or perhaps by heat alone, become an ethiops mineral.

P. 248. The cauſe of the great volatility of the mixture of vitriolic and nitrous acid is, that the former has a much more powerful attraction for water than the latter, and by depriving it of the water leaves it in an incoercible ſtate. It ſhould be added, that the nitrous acid having the ſtrongeſt attraction for phlogiſton, takes it from the vitriolic acid, and thereby increaſes its own volatility.

P. 255. Was not this powdery ſubſtance *corroſive ſublimate?* Perhaps this is an eaſier way than the common one of making that preparation.

P. 298. The ſteam of water is alſo a vehicle of ſound.

## NUMBER IV.

### *A Letter from* DR. WITHERING, *containing an Account of a new Method of impregnating Water with Fixed Air, illuſtrated with a Drawing.* Fig. 3.

Birmingham, 12th February, 1781.

Dear Sir,

I Have at length finiſhed the apparatus for impregnating water with fixable air which I mentioned to you ſome time ago; and can now aſſert from experience, that it is attended with every advantage that I expected. The incloſed drawing will ſufficiently point out to you the different parts of the apparatus, with their uſes; but for the ſake of thoſe who are leſs converſant in theſe ſubjects, I have ſubjoined a particular explanation.

I am, &c. W. WITHERING.

A. A glaſs veſſel, about 10 inches high in the cylindrical part, and 6 ½ inches diameter.

B. A glaſs veſſel, about 12 inches high in the conical part, one inch and a half in the neck, and 5 inches diameter at the bottom.

C. A copper pipe paſſing through the ſtopper of the veſſel B, and tied faſt into the flexible tube D.

D. A flexible air tight tube made of ſtrong leather, and kept hollow by means of a ſpiral wire paſſing through its whole length.

E. A conical braſs pipe, with a ſtop-cock faſtened to the tube D.

F. A conical pipe, with a ſtop-cock G; into this pipe the end of the tube E is accurately ground ſo as to be air tight.

G. The ſtop-cock cutting off all communication with the atmoſphere when the pipe E is removed.

H.H. Two large hogs bladders, each of which ought to hold two quarts.

I. A ſtop-cock to prevent the water riſing into the bladders when the veſſel A is agitated.

K. A bladder, tied to the crooked tube with the ſtop-cock,

L; which occaſionally opens or ſhuts the communication with the veſſel B.

M. A glaſs funnel, accurately fitted with the glaſs ſtopper N.

O. The aperture fitted with a glaſs-ſtopper, from which the impregnated water is to be drawn for uſe. Or inſtead of the glaſs-ſtopper, a ſilver-cock might be more conveniently applied.

P. The tube opening into the veſſel A.

USES.

## USES.

This apparatus is contrived with a view to the impregnation of water with fixable air, and with all the other ingredients which are found to exiſt in the moſt celebrated mineral waters.

In order to effect theſe purpoſes, 1ſt, fill the veſſel A with pure water, and add the other ingredients, when ſuch are required; and in the due proportion to the quantity of water, which will be about five quarts.

2d, Put into the veſſel B, as much marble or whiting, in ſmall lumps as will cover its bottom to the height of about two inches. Then pour in water to the height repreſented by the dotted line.

3d, See that all the bladders are tied round their reſpective tubes, ſo as to be perfectly air tight.

4th, Let the mouth of the veſſel A be well fitted with a cork; through a hole in this cork paſs the tube P, and upon the cork put melted ſealing-wax of the ſofteſt kind, or elſe modelling wax, ſo as to make the whole air-tight.

5th. The Mouth of the veſſel B muſt be ſtopped with a piece of mahogany prepared in the following manner. Let the wood be turned in a lathe in a conical figure, but a little larger than what the mouth of the glaſs will admit. Put this piece of wood into melted bees wax, and heat the wax until the wood begins to grow black. When cool, turn it again in a lathe until it fits the mouth of the veſſel. The tubes C, L and M are fitted into holes bored through the wooden ſtopper, previous to its being immerſed in the melted bees wax.

6th, Puſh the tubes C, L, M, through their reſpective holes in the wooden ſtopper; preſs this ſtopper into the orifice of the veſſel B, and cement the whole with ſealing, or modelling wax.

7th, Shut the ſtop-cocks I and L; having previouſly preſſed the air out of the bladder K; open the ſtop-cocks G and E; then ſqueeze the air out of the bladders H H, and afterwards preſs the conical pipe E into the pipe F.

8th, Pour about a large ſpoonful of the acid of vitriol, known by the name of oil of vitriol, through the funnel M, and ſtop it with its ſtopper N. The fixable air now let looſe by the effervеſcence in the veſſel B, riſing through the tube C, paſſes into the bladders H H, and diſtends them.

9th, When theſe bladders are diſtended, open the ſtop-cock I, and from the aperture at O, draw out about a quart of water. The ſpace formerly occupied by the water ſo drawn out will now be filled with fixable air, which preſently begins to be abſorbed by the remaining water, and is ſtill ſupplied from the bladders H H, and from the effervеſcing mixture in the veſſel B. Whenever theſe bladders are conſiderably collapſed, more vitriolic acid muſt be added through the funnel M, ſo that they may be kept conſtantly and pretty fully diſtended.

10th, If an impregnation is ſpeedily required, turn the ſtop-cocks at G and E, and open that at L. Then ſeparate the pipe E from the tube F, and agitate the veſſel A. During this time the fixable air that is produced, paſſes into the bladder K; from which it may afterwards be preſſed into the other bladders when the two parts of the apparatus are again united.

11th, During the agitation, cloſe the ſtop-cock at I, opening it only occaſionally to replace from the bladders H H the fixable air abſorbed by the water.

12th, If a ſtrong impregnation is required, this proceſs ſhould be carried on in a room, the heat of which does not exceed 48° of Farhenheit's thermometer.

### *Reaſons for preferring this Apparatus to that in common uſe.*

1ſt, It can be made at leſs expence.

2d, Should any part be broken, the repairs are much more practicable, and leſs expenſive.

3d, The whole quantity of fixable air produced is converted to uſe, and conſequently there is no waſte of the vitriolic acid.

4th, It impregnates three times the quantity of water at one time, more completely, and with leſs trouble.

5th, The water ſo impregnated for ever remains ſo, if the joints and cocks are made perfectly air-tight; and it may be drawn off at different times without injuring the remainder.

Laſtly, it may be neceſſary to obſerve that the impregnated water receives no taſte from the bladders; and that if the veſſel A with its water ſo impregnated be ſeparated from the veſſel B, at the conical parting E, F; it may be incloſed in a pyramidical mahogany caſe, out of the lower part of which the ſilver cock at O projects, and thus ſerve for an ornamental as well as a luxurious and ſalubrious addition to our ſideboards, particularly in the ſummer and autumnal ſeaſons.

N. B. In order that the cocks may continue perfectly air-tight, it will be neceſſary to ſupply them about once a year with a very ſmall quantity of unſalted lard.

Modelling wax may be had at the engravers, or it may be made thus. To half a pound of melted bees wax add two ounces of tallow and one ounce of Venice turpentine. Red lead or Spaniſh brown may be added in quantity ſufficient to give it a colour. Continue ſtirring the mixture until it is cold.

## NUMBER V.

### *A Letter from Mr.* JOHN WARLTIRE, *Lecturer in Natural Philoſophy, on the Firing of Inflammable Air in cloſe veſſels.*

Birmingham, 18th April, 1781.

SIR,

I had long entertained an opinion that it might be determined whether heat is heavy or not, by firing inflammable air mixed with common air, and applying them to a nice balance; but as I conceived the danger of paſſing the electric ſpark through ſo combuſtible a mixture in a cloſe veſſel to be greater than it is, I was deterred from making the experiment; 'till, being encouraged by you, I procured a copper ball, or flaſk, which holds three wine pints, the weight 14 oz. with a ſcrew ſtopper adapted to it, and began with ſmall quantities of inflammable and large quantities of common air, which were fired without the leaſt danger.

I then increaſed the bulk of the inflammable air to half that of the common air, which when fired made the flaſk very warm to my hand; and every time I applied a long glaſs tube faſtened to the pipe of a pair of bellows, to blow the phlogiſticated air out of the flaſk, I obſerved a ſmoke eſcape along with it. I alſo fired the air when the flaſk was under water, and did not obſerve any thing eſcape when I perceived the heat againſt my hand with which I kept the ball from riſing. When the ſtopper was unſcrewed, the external air always ruſhed into the veſſel containing the phlogiſticated air with ſome violence.

The method I uſually practice to mix the airs in any proportion, is accurately to fill a meaſure with inflammable air, and reſt it in a tub, with its rim barely under water, hanging over the edge of a ſhelf, ſo far as to admit one leg of an inverted ſyphon, the other leg being cloſed, but afterwards opened, and the copper flaſk inverted upon it, but cloſed with its ſtopper when the meaſure of air has been plunged under water, to force it out through the ſyphon. I have ſometimes exhauſted the common air to admit the inflammable air into the flaſk, but I do not find that that circumſtance produces any difference in the reſult of the main experiment.

My next object was to adjuſt the balance in ſuch a manner as that I could always be certain to weigh to leſs than a grain when it was loaded with the flaſk and its counterpoiſe, and I conſtantly examined it at the beginning and end of every experiment. The apparatus being adjuſted, I proceeded to make the experiment I had in view, and always accurately balanced the flaſk of common air, then found the difference of weight after the inflammable air was introduced, that I might be certain I had confined the proper proportion of each, the electric ſpark having paſſed through them the flaſk became hot, and was cooled by expoſing it to the common air of the room; it was then hung up again to the balance, and a loſs of weight was always found, but not conſtantly the ſame; upon an average it was about two grains.

I have fired air in *glaſs* veſſels ſince I ſaw you venture to do it, and have obſerved, as you did, that though the glaſs was clean and dry before, yet after firing the air, it became dewy, and was lined with a ſooty ſubſtance.

If you think theſe experiments worth communicating to your philoſophical acquaintance, it may be depended upon that the circumſtances appeared to me as I have repreſented them, whatever they may be found to prove.

I am, with great eſteem,

Your humble ſervant,

JOHN WARLTIRE.

The preceding article, though coming too late to be printed together with the reſt of the volume, and to be noticed in the contents of it, I have thought proper to inſert on account of the remarkable facts it exhibits.

Dr. Withering and myſelf were preſent when the mixture of common air and imflammable air was fired repeatedly in the cloſe copper veſſel, and we obſerved that, notwithſtanding all the precautions we could think of, the veſſel certainly weighed leſs after the exploſion than it had done before. I do not think, however, that ſo very bold an opinion, as that of the latent heat of bodies contributing to their weight, ſhould be received without more experiments, and made upon a ſtill larger ſcale. If it be confirmed, it will no doubt be thought to be a fact of a very remarkable nature, and will do the greateſt honour to the ſagacity of Mr. Warltire.

I muſt add, that the moment he ſaw the *moiſture* on the inſide of the cloſe glaſs veſſel in which I afterwards fired the inflammable air, he ſaid that it confirmed an opinion he had long entertained, viz. that common air depoſits its moiſture when it is phlogiſticated. With me it was a mere random experiment, made to entertain a few philoſophical friends, who had formed themſelves into a private ſociety, of which they had done me the honour to make me a member.

After we had fired the mixture of *common* and inflammable air, we did the ſame with *dephlogiſticated* and inflammable air; and though, in this caſe, the light was much more intenſe, and the heat much greater, the exploſion was not ſo violent, but that a glaſs tube about an inch in diameter, and not exceeding one tenth of an inch in thickneſs, bore it without injury. Nor ſhall we wonder at this, when we conſider that the expanſion of air by heat does not go beyond four or five times its bulk. It is evident, however, from this experiment, that little is to be expected from the firing of inflammable air in compariſon with the effects of gunpowder; beſides that after firing of inflammable air there is a great diminution of the bulk of air, whereas in the firing of gunpowder there is a production of air.

AN

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## ERRATA.

P. 32, l. 7, for *had*, read *has*.
45, l. 1 (*b*) for *proportion*, read *portion*.
65, l. 5 (*b*) dele *a*.
93, l. 1 (*b*) read *between one half and one third*.
117, l. 15, read *I then uſed*.
126, l. 6 (*b*) for *in*, read *into*.
174, l. 2, — *at*, —— *it*.
216, l. 9 (*b*) — *made*, — *made in it*.
239, l. 6 (*b*) — *of*, —— *with*.
251, l. 4 (*b*) — *vapours*, — *vapour*.
284, l. 3 (*b*) — *they*, —— *they were*.
P. 25, l. 3 (*b*) Note, for *carried in*, read *carried on in*.
(*b*) Signifies *from the bottom*.

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